Science Progress.

No. 14. April, 1895. Vol. III.

MOUNTAIN SICKNESS.

BASED ON NOTES BY MR. W. M. CONWAY OF HIS EXPERIENCES IN THE KARAKORAM HIMALAYAS.

WHEN, in mountain climbing, a height of 16,000 to 17,000 feet is reached, the generality of people experience, to a greater or less degree, a train of symptoms to which the term mountain sickness (Fr. *mal des montagnes;* Ger. *Bergkrankheit*) is applied. These symptoms become more and more distressing the more the above heights are exceeded, without however varying in kind. They may become so serious that the life of the individual is endangered. There is still a good deal that is obscure about the condition, therefore the observations made by Conway, who with his party has climbed higher than any previous traveller, and who seems a very good observer, are of interest and importance. When he left England for his expedition in the Karakoram district he took with him, at Professor Clifford Allbutt's suggestion, a Dudgeon's sphygmograph with which he obtained pulse curves at different heights up to 23,000 feet from himself and other members of his expedition. Some of these tracings were lost in transmission to England, but enough remains to teach all that we can expect to learn from such records. These curves he has placed in my hands for examination, and I will presently reproduce some of the more important of them and draw such conclusions as they seem to allow of.

On Conway's return to England I asked him to write

out his notes on the effects of height, and let me have them along with the pulse curves. I here insert these notes :—

GENERAL NOTES BY W. M. CONWAY.

Vomiting.—We had only two cases of this, both Gurkhas ; one at Footstool Camp (about 17,000 feet), the other on the Pioneer Peak, where Amar Sing had to stop at 22,000 feet, on the "Water Col". The Gurkha ill at Footstool Camp was a youth. The Gurkhas, for *caste* reasons, had to eat their ordinary food everywhere. We changed our feed as we went higher, and on the actual ascent of big peaks only ate Kola biscuits after leaving camp and before getting back to it again. I ascribe our freedom from indigestion to this precaution.

As to our general experiences I may note as follows :—

The effect of altitude came upon us all quite gradually, and appeared to affect the native coolies (born and bred at 10,000 feet), as much as ourselves. There were slight individual differences between us, but nothing at all marked. The way altitude showed itself was by our diminished pace when we thought we were going as fast as at lower levels. Referring to our ascent from Asholey to Baltoro, then up the Baltoro glacier to Junction Camp, Footstool Camp, and so to the top of the Pioneer Peak (23,000 feet), descending to Footstool Camp and then down the Baltoro glacier again to Corner Camp, Baltoro and Asholey, I note that we first became definitely conscious of discomfort between Junction Camp (16,000 feet) and Footstool Camp (17,000 feet)—a very gentle slope. I halted at Junction Camp ; Bruce and Zurbriggen went on to Footstool Camp, and it was there that the young Gurkha was sick and generally upset. Bad weather came on, and Bruce and Zurbriggen returned to Junction Camp and warned us that we should feel the altitude a short way farther up. I stayed four days at Junction Camp and then followed Bruce and Zurbriggen to Footstool Camp. I ought to have been habituated to altitude by then, but was not, and felt exactly as Bruce and Zurbriggen had felt both times they passed that way. This part of the glacier was a straight almost

level trough between two high walls of mountains right and left. Above Footstool Camp the ascent was through a difficult ice-fall. Here, when we got into enclosed places, with the sun hot on us, we felt altitude badly, when in shady places less, and in general when the sun was hot on us we always felt worse than when it was shut off by clouds, rocks, or ice. There was as we ascended a steady diminution in our powers, a desire to keep the arms from hanging by our sides, a disinclination to start observing instruments or doing anything. We had to breathe steadily, the least holding of the breath (to observe an instrument, or take a long stride), being instantly punished by giddiness and puffing. The higher we got the more conscious were we of our hearts—unable to sleep on our left sides, tendency to palpitation even at night. Our feet easily and rapidly got cold and lost sensation. By night we were always more comfortable than by day, and in cloudy than in fine weather. We could have done much more by night if our feet had been able to hold out against the cold. On the peak we felt much worse on slopes than on the arête; we had difficulty to restrain ourselves from taking to the "cornice". We often felt in hollow places and even on slopes the sensation as though the air was *stagnant*. The night at the highest camp *after* the ascent of Pioneer Peak was our worst night; palpitations constantly woke me up. In the descent we felt discomfort (such as was experienced in the ascent between Junction and Footstool Camps) down to a much lower level, as far as Corner Camp (13,000 feet), than we had felt in the ascent. We seemed to become continuously *less* able to hold out against altitude, the longer we remained at a high level. I several times took my temperature, but never hit it at anything but normal. I did not take it on Pioneer Peak. I have said that I thought our appetites remained as usual. I think that I was in error. We did not eat so much at the two highest camps, but we should have eaten more than we did if we had had more to eat. I can't say how, in that case, we should have been affected.

The ages of the European members of the party were—W. M. Conway, 36; M. Zurbriggen, 34; J. McCormick, 33; J. H. Roudebush, 21; O. Eckenstein, 34.

With regard to the height at which distinct symptoms first show themselves, Conway's account agrees very well with the experiences of the Schlagintweits, (1) who had very extended experience and moved with fairly large parties of natives among the Himálaya, Karakoram, and Kuenlúen districts, and who noted that "for the generality of people the influence of height begins at 16,500 feet, a height nearly coinciding with that of the highest pasture-grounds visited by shepherds." Conway's party first became conscious of discomfort between 16,000 and 17,000 feet. Whymper's (2) expedition to the Great Andes of the Equator first felt real discomfort at 16,664 feet.

For healthy men in good training who take carefully chosen food we may therefore accept the Schlagintweits' limit as fairly accurate, while for people who are not in good condition, or who take food which they cannot digest, the limit may be very greatly lowered—a point which I will presently consider. This lower limit of distress in mountain climbing is therefore put wonderfully closely by the three sets of observers named. Whether it can be raised by prolonged residence at high levels is, I think, doubtful, and on this point it is not easy to obtain information of much value seeing that in the absence of exact measurements observers may get so accustomed to what at first distressed them as to undervalue it.

The symptoms of mountain sickness as described by a very large number of observers (3) may be stated as follows, keeping in mind that some only of them are noted by each writer.

Those constantly met with are great difficulty in getting enough air into the lungs, and a corresponding feeling of anxiety and distress, together with greatly increased dyspnœa and fatigue on exertion and lassitude when at rest. The respirations are rapid, short, and gasping, and the ability for active exertion is much lessened, all these signs being met with in animals as well as in man.

Common symptoms are palpitation of the heart with a quickened pulse, severe headache, giddiness, singing in the ears, diminished appetite, nausea with or without vomiting, bleeding at the nose, and coldness of the extremities. Fairly common are indifference to danger, and loss of interest in things generally, often with a tendency to somnolence—although in some there is increased excitability—spitting of blood due to hæmorrhage from the lips, gums, air passages, or lungs. Bleeding from the conjunctivæ is sometimes observed, as also blurring of the sight, diarrhœa, and, in severe cases, muscular weakness so great that standing or sitting up is no longer possible; eventually the limbs may become completely paralysed, and there may ensue loss of consciousness, and death.

Curiously enough, lividity of the face, and especially of the lips, which must be familiar to every Alpine climber at heights of 12,000 to 15,000 feet, is apparently overlooked by most writers or wrongly ascribed to cold.

The first question I have to consider is whether these symptoms are referable to one condition—*viz.*, insufficient supply of oxygen, or whether this may be complicated by other conditions which require to be taken into account. If they be all due to dyspnœa or asphyxia alone they include phenomena which are not generally recognised by pathologists as possible results of imperfect supply of air, while if they be complicated with some other result of the rarefaction of the air, it is desirable that these possible complications should be clearly recognised.

How far do the known results of dyspnœa on the animal body correspond with the phenomena above referred to?

The gasping respiration we know well as the constant result of imperfect supply of air to the lungs when the air passages are unobstructed. We may equally safely put down the muscular weakness to imperfect supply of oxygen which we know is necessary for the contractions of the muscles.

The heart symptoms—*viz.*, the palpitation and the acceleration of the rhythm—may safely also be ascribed to the dyspnœa. On arresting the respiration in the lower

animals there is at first powerful vagus action, and the auricular beats soon become so weakened that the ventricles take on their own independent rhythm. It cannot in the meantime be proved that this causes the subjective feeling of palpitation of the heart, and it can only be said that the independent ventricular contractions show themselves under conditions similar to those which produce the feeling of palpitation of the heart, and that the latter is just the kind of feeling one would expect when the heart changes its mode of contraction in this particular way. In asphyxia as produced in animals there is a very great dilatation of the ventricular walls although the strain upon them (measured by the amount of blood pumped out *plus* the pressure in the aorta and pulmonary arteries) may not have been increased. This yielding of the ventricular wall is almost certainly due to the diminution in amount of oxygen in the blood circulating in the coronary arteries (4).

This weakening of the heart brings the vagus mechanism into increased activity, the effect of which on the heart is to economise its action; and palpitation, if I am right about its nature, is under the circumstances an indication that the heart is distressed.

To the cardiac dilatation due to asphyxia is added the dilatation which is caused by muscular exertion; the two together will lead to heart-failure more readily than either of them singly; by heart-failure being understood the condition in which from its over dilatation the tricuspid orifice can be no longer closed by the tricuspid valve.

There is no doubt that heart-failure is of not uncommon occurrence even in the Alps (5), and that when it occurs the person suffering from it is practically incapacitated from further climbing owing to intense dyspnœa, palpitation, and irregularity of the heart. Guides, who know the condition perfectly well in their own cases, or in that of their "Herren," have told me that when badly taken with it near the top of a mountain, the only way to get the person up the last 100 feet or so, is to practically carry him up. The incapacity for further exertion is certainly very great, as I happen to know, but a quarter or half an hour's rest

generally makes the person feel all right again, recovery beginning to take place in ten minutes or so, although at first, sitting down gives no relief to the distress. I must return to this subject later.

I now come to the other symptoms of mountain sickness, and have to consider how far they can be looked upon as due to asphyxia. The headache, with a sense of fulness, as if a tight band were drawn round the head, may be safely put down to cerebral congestion, and the hæmorrhages from the nose and conjunctiva agree with the known fact that the blood supply of the latter usually goes with that of the brain. Cerebral congestion is a constant result of asphyxia (7). The giddiness, nausea, vomiting, blurring of the sight, and the indifference to danger, somnolence, and perhaps also the excitability, are all, I imagine, caused by the diminished supply of oxygen to the brain, which is perfectly compatible with congestion of that organ if the tension of the oxygen in the blood be reduced. The nausea and vomiting are of cerebral origin, for they do not cease with evacuation of the stomach, although they may follow the ingestion of food, and may become more marked as a result of indigestion. The lessened appetite and diminished power of digestion are presumably due to the anæmia of the digestive tract, which are invariably produced by asphyxia, according to observations by L. Cobbett and myself, which are as yet unpublished. The singing in the ears may be due in part to difference in pressure of the air within and without the tympanic membrane, the Eustachian tube not completely fulfilling its function in this respect.

The muscular weakness I have already referred to as due to the imperfect supply of oxygen to the muscles. The ready cooling of the extremities which prevented Conway's party from moving by moonlight at high altitudes I am inclined to put down to the diminished heat production, which results from the imperfect supply of oxygen to the muscles, which are the chief sources of heat in the body, and which may be aided by congestion of the skin increasing the amount of heat given off by it.

So far as the symptoms are concerned, we need therefore

be in no difficulty regarding the nature of mountain sickness. It is asphyxia. The important feature in the asphyxia of mountain sickness is the reduction in the amount of oxygen supplied to the tissues, but the same effect is produced if, with a limited supply of oxygen to the system, there be from any cause an increased demand for it by the tissues. Muscular exertion is the most familiar example of this, and the greatly increased distress which results from attempts at active movement, is one of the features of mountain sickness. There is another cause of increased demand for oxygen by the tissues which is of importance in connection with mountain sickness, *viz.*, fever. This is well illustrated by Whymper's experiences on Chimborazo (*loc. cit.*), where at a height of 16,664 feet he and two others of his party had an alarmingly acute attack of what they considered mountain sickness. This came on suddenly after a meal, one of the four members of the party being unaffected. Those affected felt feverish, thirsty, and had intense headache with extreme dyspnœa. These symptoms lessened a good deal next day, and two of the party were able to climb on the third day. No such intense distress was experienced by any of them on climbing more than 3000 feet higher. Whymper recognises that this attack must have had a temporary cause, but he appears to have overlooked the fact that fever is not present in true mountain sickness, and that in all probability it was a case of poisoning with putrid food. Whymper mentions that some of his tinned meat had gone wrong, and it is curious that he should have overlooked the possible connection of this with the attack from which he and the Carrels suffered on Chimborazo. The serious symptoms of asphyxia are only what would naturally be expected to accompany any rise of temperature of the body at high altitudes.

I now come to the real gist of the matter, the question which Professor Clifford Allbutt hoped would be decided by Conway's observations with the sphygmograph, namely, whether heart-failure is an essential element, indeed the essential element, in mountain sickness? Asphyxia, from whatever cause, can produce cardiac weakness and dilata-

tion, while, on the other hand, heart-failure, owing to the imperfect circulation, and therefore imperfect aeration of the blood, is a common and well-recognised cause of dyspnœa or asphyxia. If dyspnœa or asphyxia leads at altitudes of between 16,000 and 17,000 feet to heart-failure on moderate exertion, a vicious circle is produced, since the asphyxia tends to produce heart-failure, and *vice versâ*.

If this be the case, the limits of mountain climbing, as regards height which can be reached, become more restricted than would be the case if we had to deal with asphyxia alone.

To state the point about heart-failure more clearly, let me insist that the heart is a pump, which must pass on in healthy man or animals an amount of blood, which varies greatly at different times, in order to meet the requirements of the system as a whole. If the heart's work (expressed by the quantity of blood passed on, and the hydrostatic pressure against which this blood is expelled) be increased beyond a given limit, the heart as a pump becomes inefficient from dilatation of the ventricles, so that the valves between the latter and the auricles, can no longer close completely. This inefficiency of the cardiac pump can be produced in two chief ways. Firstly, by increasing the work thrown upon the heart ; and, secondly, by diminishing the power of the heart. The diminution in power of heart can be produced by a great many causes, one of which is imperfect supply of oxygen to the organ. Others are, alcohol, tobacco, tea, disease of the heart, etc. Increase of work may be given by muscular exertion, fever, mental excitement, etc., etc. Any one of all the above causes is capable by itself of producing heart-failure. In most cases two or more of them are combined. For example, there can be little doubt, from the observations of Clifford Allbutt and others, that many of the cases of the mountain sickness which occurs at low altitudes, and which has so often been described in the Alps and elsewhere, are really cases of heart-failure from over exertion and imperfect training, combined perhaps in some cases with error in diet.

Indeed, heart-failure which cannot be distinguished from true mountain sickness may occur at the sea level. The point we have to consider, then, is how far Conway's observations and pulse-tracings indicate whether or not, while suffering from true mountain sickness, there was also heart-failure.

I give here reproductions of the pulse tracings of Conway and his party, taken at different altitudes, having chosen twenty-one of the total twenty-seven curves which have been submitted to me, those omitted being taken at lower levels than those shown, and being therefore of less interest in the present connection. The letters to the left of the figures show from whom each tracing was obtained, and the numbers give the pulse-beats per minute, which I have calculated from the rate of movement of the clockwork used to move the paper past the point of the recording lever. The descriptions to the right of the figures are Conway's notes. I will analyse the tracings briefly, without seeking to confine my remarks to the question as to whether they do or do not indicate that heart-failure is an essential element in mountain sickness, and I make allowance for the inaccuracy of the sphygmograph employed.

Of the curves in Fig. 1, Conway's and McCormick's are ordinary tracings, such as one usually obtains from healthy men with a Dudgeon's sphygmograph, and the pulse-rate is within the usual limits of quiet life. Roudebush's and Eckenstein's show a more rapid beat, with the change in the form of the pulse-wave which usually goes with it.

Fig. 2 shows that the four members of the party were fatigued, the quick pulse rate in the absence of fever indicating a demand by the tissues for more blood. I cannot go over the reasons which exist for believing that the "nervi accelerantes cordis" have for their function to increase the force and frequence of the heart in response to demands of the rest of the body for more nutriment (4), and need only note that the form of the curves corresponds with the increased rate of heart-beat. It can be seen, however, that at the elevation of 14,000 feet the hearts of

the four individuals concerned were beating very rapidly indeed—more than was the case with the same persons when they had climbed 8000-9000 feet higher. From this I conclude that fatigue or excitement, or both, rather than the rarefaction of the atmosphere is the cause of the quick pulse rate which has been associated with mountain sickness. Figs. 4-12 show that at heights of 17,000-23,000 feet the heart-beat need not be greatly quickened.

Fig. 3, showing tracings taken at 16,000 feet, reveals nothing abnormal in the form of the pulse curves. Fig. 4, at 17,000 feet, shows curves which one may get any day at sea-level.

Fig. 5, at 18,000 feet, cannot be called at all unusual. Fig. 6, at the same height, may be called a good healthy pulse. Fig. 7 is more dicrotic than is usual, but is not abnormal. Fig. 8, at 20,000 feet, with a fall to 48 heart-beats per minute, shows vagus action, the ventricles missing every alternate beat of the weakened auricular contractions. It is one form of the pulse of a distressed heart and the form of the wave corresponds. Fig. 9, in Conway's case, looks like an anacrotic pulse wave, which is generally, but not always, associated with high arterial pressure; but in any case Zurbriggen's tracing at the same height is not one of high pressure. Figs. 11 and 12 are of not unusual form, and the rate of heart-beat is slowed down to about the normal in unfatigued individuals.

It may be said at once that these tracings do not enable us to decide the question as to whether heart-failure is or is not an essential element in mountain sickness. Two of them show unmistakably increased vagus action on the heart, but the others show rather evidence of fatigue than anything else. The vagus action is a sign of a distressed heart, and is usually if not always present in heart-failure, but it is often enough brought into play long before the heart has become overstrained. The palpitation of the heart which is so commonly noted in mountain sickness and exertions is, as we have pointed out, another sign of a distressed heart. It must be remembered that the tracings were taken during periods of rest, when the rare-

faction of the air alone, and the consequent diminution of the oxygen in the blood, was the only influence tending to weaken the heart.

During such periods of rest, the comparative regularity of the tracings is against the likelihood of failure of the heart at such times, and this is pretty well all one can say about them. They do not, and they cannot, enable us to decide definitely whether the heart is overstrained *during the exertion*, which is a necessary part of mountain climbing. I know of only one means by which this important question can be definitely decided, and that is by using a stethoscope, and learning whether or not a heart-murmur accompanies the first sound of the heart during exertion. If it does, we may diagnose functional incompetence of the mitral or tricuspid valve, or both, and therefore the failure of one or both ventricles ; if the murmur be not present under these conditions we must in the meantime exclude heart-failure.

Conway's observation that more distress was experienced in hollow places than when walking on an arête, confirms what has been noted by others. This may be due to the fact that water takes up more oxygen than nitrogen from the air, so that when, on a high peak, the sun falls upon the snow, melting a certain part of it, the neighbouring air is robbed of some of its oxygen. The wind on a ridge need not have passed over fields of melting snow, and may therefore contain a larger percentage of oxygen. The difference in the amount of oxygen compared with nitrogen which water obtained from melting ice takes up is not very great, and the fact is probably not of much importance, but seeing that the two gases will not be dissolved in the proportions in which they exist in the atmosphere, but will each be dissolved as if it were the only gas present, we may have, even if the melting snow takes up equal volumes of oxygen and nitrogen, a very serious lowering of the percentage of oxygen in the air which rests or passes over the melting snow. This offers to my mind a very probable explanation of the increased distress felt by Conway in hollow places, especially when the sun was shining on them.

Conway does not, in his notes, refer to variation in the

degree of distress with the direction of the wind ; but this is a subject which was much noticed by the Schlagintweits, and by numerous other observers, who found that they were most distress when there was a wind from some particular direction. There is no reason for supposing that this was due to temporary variations in the barometric pressure—as such an obviously possible explanation could not have been very easily overlooked by the Schlagintweits. It is certain that air which has passed over a large area of snow melted by the sun will have suffered a diminution of its oxygen. If this idea be correct, such winds ought to be most distressing in the afternoon when the quantity of water set free by the melting of the snow is greatest. On being frozen again at nightfall, the melted snow will give off relatively a large proportion of oxygen, which may explain why Conway and his party felt so much better at night. It is possible that the increased distress felt by Conway and others in sunshine as compared with shade or cloudy weather, was in part due to the increased demand for oxygen by the body when warmed by the sun's rays. Another point that strikes one in going over the literature of Mountain Sickness is that sometimes, especially in the Andes, dangerous symptoms show themselves at altitudes below 17,000 feet in persons riding on horse or mule-back, and where there is therefore no question of over-exertion. I have been at pains to follow out some of these cases, and find that previous attacks of malarial fever are recorded. The diminished amount of hæmoglobin in the blood which follows malaria will, in these cases, readily account for the asphyxia which shows itself, and may cause death in men and animals at heights below 17,000 feet.

While thanking Conway most cordially for all he has done in the past in adding to our knowledge about mountain sickness, I would venture to ask him, when next he goes mountain climbing above 17,000 feet, to purchase before leaving England a binaural stethoscope, and get some medical friend to put him in the way of recognising a heart-murmur, and to use this stethoscope on himself and others of the party during or *immediately* after exertion.

One other point remains, and that is, how far, judging from the observations and tracings which are reproduced above, Conway had reached the highest climbable altitude. During rest at least, even at 23,000 feet, the curves indicate on the whole, that muscular fatigue and distress of the heart were present rather than the nearly complete collapse of muscular power which accompanies well-marked heart-failure with the vagus slowing and irregularity. There is, therefore, no obvious reason why they should not have gone higher, if they could do it quietly enough and if they could choose their own times for going on and camping, etc., which of course is the real difficulty. The curves show that they were in a condition to go on, and they agree with Conway's own feeling that they had not come to the end of their tether.

BIBLIOGRAPHY.

(1) H., A., and R. DE SCHLAGINTWEIT. *India and High Asia*, vol. ii., p. 484. London, 1862.

(2) E. WHYMPER. *Great Andes of the Equator*. London, 1892.

(3) For literature up to 1887, *vide La Pression Barométrique*, Paul Bert. Paris, 1877.

(4) ROY and ADAMI. Contributions to the Physiology and Pathology of the Mammalian Heart. *Philosophical Transactions*, vol. clxxxiii., p. 284, 1892.

(5) T. CLIFFORD ALLBUTT. *St. George's Hospital Report*, vol. v., 1870.

ROY and ADAMI. On Heart Overstrain. *British Medical Journal*, 15th December, 1888.

(6) ROY and SHERRINGTON. On the Regulation of the Blood Supply of the Brain. *Journal of Physiology*, vol. xi., p. 85.

CHARLES S. ROY.

SPANISH ANTHROPOLOGY.

THE physical anthropology of the Peninsula was unti quite lately a kind of *terra incognita*, which may even still be said of that of Portugal; but the reproach has been wiped away from Spain pretty completely within the last five or six years.

What knowledge we had prior to 1889 on the subject had no scientific basis, except so far as the Basques were concerned. Broca's invasion of Guipuzcoa, or rather his acquisition, through Dr. Velasco, of the famous collection of skulls from Zaraus, had shown that there at least mesokephaly of a particular type preponderated, and that the old hypothesis of their affinity with the Lapps, based as it was upon hasty philological conclusions, was not at all borne out by craniometry.

Dr Landa[1] followed with some observations of the kindred people of Navarre, but nothing of importance was added until 1889, when the brilliant little work of Telesforo de Aranzadi appeared. This was based on the principles of concentration and thoroughness; in locality it was confined to Guipuzcoa and a small part of Biscay, in numerical extent to 250 young soldiers; but within these limits it extended to stature, headform, facial angle, nasal index, proportion of features, proportion of limbs, colour of hair, eyes and skin, and association of most of these with each other.

In his next important work De Aranzadi was associated with De Hoyos-Sáinz. It was a brief account of the distribution of the kephalic index in the provinces of Spain, founded on the examination of 478 modern crania, of which 423 were sufficiently perfect to yield also the nasal index. Unfortunately 14 of the 48 provinces were wholly unrepresented in the collections to which the authors had

[1] It is worthy of note, as a witness to the accuracy of both observers, that Landa, who measured 63 persons in Navarre, and Olóriz, who examined 112, made the average (live) index 78·32 and 78·40 respectively, thus agreeing within one tenth of a degree.

access, while in 16 others the number of crania was less than 10. These deficiencies, both total and partial, are mostly in the east and south-west, the whole of Cataluña and several provinces of Aragon, Valencia and Lower Andalusia being awanting. The Basque provinces, Asturias, Galicia, Segovia, Cuenca and Toledo, are best represented.

A still more markworthy work is that of Professor Olóriz, which has but recently appeared. It is a bulky and closely-printed memoir on the geographical distribution of the kephalic index in Spain; and seldom has a limited though important subject been so thoroughly worked out as this has been by Olóriz. One cannot but be reminded by the labours of these men of the most conspicuous of their national virtues; De Aranzadi representing the adventurous valour of the Basque, Olóriz the dogged, unconquerable, Numantine perseverance of the Spaniard.

Olóriz's conclusions are deduced from the measurement, in the great majority of cases by his own hands, of 8368 living heads. Having ascertained experimentally that with a total of 100 observations the possible error never reaches unity, and is usually a very small decimal, he took care to secure at least 100 from every province.

Incidentally, in the course of the definition and explanation of his methods, he develops several very important points.

Thus he shows that the mean error of a single observer (himself) is about 0·7, and the maximum error about 1·3 degree of the kephalic index. The error is apt to be greater, as might be expected, in the length measure than in the breadth measure. The mean extent of error, or divergence from the mean result, when the same head is measured by ten skilled observers, amounts to 1·78, or about one degree and three-quarters in the kephalic index, the maximum error rising to three degrees. At the first blush, such a result would almost lead one to distrust, and indeed to absolutely discredit, all series of measurements of this kind except those taken by the same pair of hands. This of course would be a mistake: such errors more or less balance each other in long series and where the

observers are many; and even the maximum error here recorded would not transpose a head from the dolicho- to the brachykephalic category. But Olóriz's experience furnishes a new caution as to the importance of the personal equation.

The mean index for Spain in general is ascertained to be 78·18, or, with correction for the different relation of the numbers observed to the population of the several provinces respectively, may be put at 78·23, which may be taken as the standard index of the country.

De Aranzadi and De Hoyos-Sáinz nowhere state their mean cranial index for the whole country, but their figures indicate that it should be somewhere between 76 and 76·5. And this is probably a trifle too high, because their material, as already stated, has been drawn in greater proportion from the north, centre and south-west than from elsewhere; and the north, as we shall presently see, is decidedly more brachykephalic than the east, or indeed than most other parts of Spain.

Some light is thus thrown on the vexed and important question of the relation of the breadth indices in the dry skull and in the living head respectively; and the practice of those who are accustomed to subtract 2 from the kephalic to form the cranial index receives some support. Olóriz himself, too, comparing 116 modern skulls found in certain known localities in Spain with 151 heads of modern inhabitants of the same, brings out indices of 76·4 and 78·8 respectively, thus showing a difference exceeding 2. Certain (95) ancient skulls, from the same or neighbouring localities, yielded an index of 75·57, distinctly lower than that of the moderns in five cases out of six; on the other hand, De Hoyos-Sáinz affirms that the modern Asturians have heads narrower by three degrees of the index than their ancestors of the sixteenth century! I say *ancestors*, because I do not believe that any considerable immigration has occurred which may have modified the breed. On the whole, therefore, there is not much to support the doctrine of the general increase of brachykephaly.

As for the opinion of Ammon and De Laponge, that cities

attract the dolichokephals,[1] Olóriz's figures are inconclusive. The Madrileños are indeed slightly more dolichous than the people of the rest of Spain, or of the surrounding rural district, or of the province of Madrid. Their index is 77·87. But if it be so at Madrid, and at Barcelona, and at Zaragoza, it seems to be otherwise at Granada and Valencia, and doubtful at Seville and Malaga.

Again, as to the relation of stature to headform, it would appear that the superiority in stature of the dolichokephals, which is the rule in some parts of Northern Europe, does not obtain in Spain. Taking the Madrid subjects (mostly recruits) for this purpose, Olóriz found no very distinct difference between the tall and the short in kephalic index —nothing beyond the limits of possible errors. If anything, tall stature and *brachykephaly* went together: 50 tall men had heads on the average 0·57 broader in index than 50 dwarfish ones.

Age appeared to be a condition of some, though of course limited, importance. 470 recruits of Madrid gave a mean index of 77·93; but 50 older men (from 30 to 74 years) gave 77·48, or nearly half a degree less. The probable causes of this discrepancy are loss of hair and shrinking of the soft parts.

Of health and sickness, again, the effects were tested.

Fifty artisans in hospital, with a mean age of 38 years, gave a mean index of only 77·16; while 50 healthy soldiers of the same class, with a mean age of 21, gave 77·67, a difference of just half a degree. To account for this Olóriz suggests that difference of age, comparative scantiness of hair, and thinning of the soft parts, may all have been operative; while difficulties of observation from constrained posture, etc., may have made the measurements of the invalids less reliable.

As for the effect of social selection, Olóriz's figures are

[1] Perhaps this is not fairly put. It is the blond dolichous type which Ammon and De Lapouge credit with the qualities that result in this attraction; I do not think they have said the same of the Mediterranean dolichos.

interesting, but inconclusive. Thus he found among his 470 recruits born in Madrid :—

No.		Stature.	Keph. Index.
100	Intellectual occupations . .	1622 mm.	78·268
319	Manual „ . .	1602	77·838
51	Mixed (shopkeepers, servants, etc.)	1614	77·984
Total 470	„ „	1607	77·934
		= 5 ft. 3·27 in.	
163	Province of Madrid		78·635
8368	Spain		78·23

Again, in Granada (city)—

34	of superior culture	78·212
33	artisans and soldiers	78·409
23	criminals	78·547

But in the province of Granada—

126	honest men	78·080
106	criminals	77·680

And in the province and city combined—

384	miscellaneous	77·861

These variations, it will be observed, are within the possible limits of error, and the more certainly so because an unusually large proportion of the Granada measurements were not taken by the author's own hand. The inferences I should be disposed to draw would be, first, that the cultivated class, being the most migratory section of the population, has naturally enough an index equal in both the cities, and identical with that of Spain in general; second, (and this is Olóriz's view of the matter), that in relation to the kephalic index all agencies are unimportant compared to what we call "race".

Following up the same careful method of appreciating all possible influences, he next tabulates the divisions of the population in relation to their geological habitat. The result is distinctly negative.

			Obsns.	Index.
Azoic	rocks	50 judicial divisions	635	78·51
Palæozoic	„	51 „	616	78·80
Secondary or Mesozoic	„	57 „	1074	78·06
Tertiary	„	97 „	1527	78·16
Quaternary	„	17 „	329	78·56

The differences, it will be observed, never attain to unity. Moreover, if each class of rocks is divided among its provinces, internal differences of much greater extent come out in every case; thus five quaternary districts in the north yield a mean of 76·87 only, while six in the south give one of 79·51. The proof goes far towards being complete; I could wish, however, that Olóriz had ascertained the relative position of calcareous and of silicious districts. He has candidly stated some apparent exceptions to the rule of the nonconformableness of geological and anthropometric phenomena. They are all, I think with him, explicable on hypotheses of race-migration. Thus the isolated granitic district of Vitigudino, on the frontier of Portugal, is equally isolated by its extremely low index (75·46) from all the seven surrounding districts of the province of Salamanca, which range upwards from 77·48 to 79·38. Its people are probably the descendants of a dolichokephalic (Atlantean or Iberian) tribe, who were driven to take refuge in an infertile granitic region by the invasion of the Kelts.

The next question put is the possible influence of the mountainous or level character of the districts, irrespective of their elevation above the sea, some of the tablelands of the interior being of great elevation. The general result is negative (mountains 78·24, tablelands 78·34); but the local differences, explicable by race-migrations and occupations, are notable and interesting. Roughly speaking, the populations of the Asturo-Cantabrian chain and the mountains of Toledo, and the plains of New Castile and Lower Andalusia, tend towards brachykephaly; while those of the southern and eastern mountains, and the plains of Old Castile, Leon and Aragon, are inclined towards dolichokephaly.

An elaborate inquiry into the difference in this respect between the several hydrographical basins of Spain leads only to the conclusion, by the author, that these local differences are usually greater in proportion to the difficulty of communication and intercourse imposed on the inhabitants of contiguous valleys by the intervening mountain-ranges.

The last of these preliminary investigations, that into the differences of indices prevailing at high and low levels, is of importance in view of the general prevalence of brachykephaly in the populations of the mountain-masses of Central Europe, which is so distinct that some authors use the name "Alpine" to designate the great round-headed race which is usually called Keltic, Rhœtian or Kelto-Slavonic; in view, too, of the opinion shadowed by Johannes Ranke as to the connection of mountain habitat with brachykephaly, and of the fact that mountains, in cold or temperate latitudes at least, are usually the refuge of primitive and conquered, plains the possession of more recent and conquering tribes.

Olóriz's facts mostly agree with the latter, not with the former, rule. A comparison of the populations dwelling below and above the 500 metre line is not very striking. In Eastern Cataluña the broader heads are in the higher ground, contiguous to the French frontier. In most parts of Andalusia the converse obtains very decidedly, the mountaineers being the longer-headed by about 1·27 degree (77·84 to 79·12). On the northern, or Cantabrian slope, the mountaineers, again, are rather broader-headed (80·2 to 79·4), which is difficult to explain, unless the invaders came by land.

But when the dividing line is drawn at 1000 metres (3300 feet), above which height there is a considerable population, the contrast is more striking. The figures are:—

	Obsns.	Index.
Below 500 metres . . .	2770	78·45
Between 500 and 1000 metres .	4213	78·20
Above 1000 metres . . .	437	77·49

As Olóriz puts it, if Spain were suddenly submerged to the 500 metre line, the index of the survivors would not be very much lower than that of the population had been previously; but if it were sunk another 500 metres, the archipelago which would have replaced the present peninsula would be inhabited solely by dolichokephalic communities, except only one or two islets in the north,

where some relics of the brachykephalic element would survive.

Examining now the 48 provinces, he finds that the mean index ranges from 76·7 in Alicante to 80·9 in Oviedo. This is a much smaller range than occurs among the French departments or the Italian provinces. And the individual variations are also less. Cadiz, Oviedo, Lugo, and in a less degree Leon, Huelva, Alava, Badajoz (!), Cordova, have widish variations, but none over 20 degrees, excepting stragglers. In Oviedo and Lugo only does the centre of the series exceed 80. In Alicante it is 76·5, Castellon 76·9, Leon 77, and Soria 77.

The trustworthiness of these figures, as bases of argument, may be tested to some extent by comparison with those of Aranzadi and Landa for Guipuzcoa and Navarre. In the former case Olóriz's figures are a little the lower. Aranzadi's Basques gave him a mean of 79·15; Olóriz's Guipuzcoans one of 78·75; Landa's Navarrese one of 78·32; Olóriz's one of 78·4. These discrepancies are not important, but they are probably not merely fortuitous. It has been shown that the index tends to diminish with age. But Aranzadi among the Basques, and Olóriz among the Navarrese, operated on the younger men, so that the disagreements may be partly explained.

It is difficult to carry out the comparison between the measurements of Olóriz's living heads and Aranzadi and Hoyos-Sáinz's skulls, because, as has been already explained, the numbers of the latter, though ample as a sample of the country, are in comparatively few provinces sufficiently numerous to yield a provincial average on which we may confidently rely. For greater security the authors have, in each case, agglomerated the provinces into regions, but these regions are not identically constituted, though there is a rough correspondence; and the following table must be read with this proviso. In every case I have added the regulation 2·0 to the cranial measurements of Aranzadi and Hoyos-Sáinz, as well as of Olóriz, and it does not seem to be too much.

Regions or Provinces.	Crania.				
	Olóriz.	A. & H.-S.	No.	Olóriz.	No.
Guipuzcoa (prov.) .	78·75	78·8	75		
Cantabria . .	79·9	80	129		
Galaica (W. Galicia) .	78·9	79·1	23		
Leon (prov.) . .	77·5	76·8	7		
Carpetania . .	78	78·1	63		
Keltiberia . .	77·7	76	84		
Oretania . .	78·6	78·9	43		
Turdetania . .	78·4	76·5	20		
Aragon . . .	77·2	?			
Valencia . .	76·8	?			
Cataluña . .	78	?			
Baleares . . .	77·7	78·3	6		
Cuenca . . .	78·5	76	51	78·8	20
Almeria . . .	77·7			77·2	42

On the whole, the correspondences are good, even where the numbers are too small for anything like certainty; but there are two very discrepant items on which it may be as well to dwell for a moment.

Not only is the number of crania from Turdetania (Andalusia) insufficient; but those tabulated come mostly from the eastern and more dolichokephalic provinces.

The figures for Keltiberia, and especially those for Cuenca, which is one of the provinces of that region as limited by Aranzadi and Hoyos-Sáinz, remain inexplicable to me. The numbers are not apparently insufficient, and a smaller series from Cuenca measured by Olóriz accord well enough with the measurements taken by him from the living population. I can only suggest that Cuenca is far less homogeneous in respect to kephalic index, according to Olóriz's data, than most of the provinces, as it is also in its hydrography; the northern and eastern more mountainous districts on the Guadiola and Cabriel rivers running below 77; while the central and western tablelands exceed 79, the former assimilated to the neighbouring Valencia, the latter to the Oretanian region.

If we now take Olóriz's map, checking and supplementing it by that of Aranzadi and Hoyos-Sáinz where available, we shall find that geographically Spain will fall into four, or perhaps I should say five, pretty distinct and

extensive regions, besides some smaller and more doubtful ones. These five are :—

First, the Cantabrian zone on the north, occupying the territory between the sea and the western prolongation of the Pyrenees, and in some parts, in Navarre, Alava and Riona, extending across the mountains southwards. It includes Asturias, Santander, Guipuzcoa, Viscaya, Alava, and most of Navarre and Galicia, and contains everywhere a considerable proportion of brachykephals, which, however, diminishes towards the east, in the Basque provinces and Navarre. The nasal index is narrow in the latter provinces, but tends to breadth further west.

The second zone corresponds roughly to the valley of the Douro, or to Leon and Old or Upper Castile. The indices are moderately dolichokephalic and leptorrhine, Aranzadi thinks mesorrhine in Leon, but the number of observations is insufficient.

The third or Oretanian zone, or that, roughly, of New or Lower Castile, occupies the basins of the Tagus and of the Upper Guadiana. Both the kephalic and the nasal indices are distinctly larger in this than in the preceding (Carpeto-Leonese) zone; as Aranzadi puts it, this is a kind of repetition of the Cantabrian, though the tendency to brachykephaly, or the proportion of that element, is not so great.

Another well-marked region, the Aragono-Valencian, or we may perhaps venture to say the true Iberian, overspreads most of the eastern part of the peninsula, from the Central Pyrenees almost to the Cabo de Palos and the frontier of Murcia. The kephalic index is narrow, especially in the three provinces of Valencia, and the nasal index is probably very leptorrhine, but this is uncertain, as we here lose the guidance on this point of Aranzadi and Hoyos-Sáinz. The central and loftiest part of the Pyrenees forms one of the best-marked anthropological frontiers in Europe, the index of the Aragonese on the south being about 77, and that of the Gascons and Languedocians on the north about 84. But further east the barrier is not so complete, and accordingly the Catalonians differ a little from the

Aragonese and Valencians in the direction of greater breadth of head.

Andalusia remains unclassified. Taking it on the large scale, Upper or Eastern Andalusia is largely dolichokephalic (Jaen 77·36, Almeria 77·76, Granada 77·86, Cordoba 77·97), and Lower or Western Andalusia tends the other way, with provincial indices from 78·34 (Seville) to 79·47 (Cadiz). Apparently Lower Andalusians are platyrrhine, but the numbers are insufficient for the conclusion.

The key to Spanish ethnology has been generally supposed to be hidden in the Basque provinces, and Aranzadi has made strenuous efforts to find it there. As Olóriz has endeavoured to beat out one single point of certainty by dint of patience, wide inductions, and careful exclusions, so has Aranzadi striven to find out great truths by the most thorough sifting of a small quantity of good material.

Taking 250 conscripts, all natives of Guipuzcoa or the neighbouring (eastern) portion of Biscay, and by speech, surname, and known descent mostly ascertained to be pure and genuine Basques, he got from them almost all the measurements, whether of head, face, body or limbs, that are generally in use among anthropologists. He draws a portrait of the average Basque, which I will reproduce, though not at full length; for it is exceedingly minute, and must have come from one who could see as well as measure. The Basque at the military age has a stature of 1640 mm. (64·56 inches), which is superior to that of the Spaniard of Madrid: his shoulders are broad and square, his fathom large (about 105), the back sinuous and hollow, the extremities rather large, the instep high, the gait straight and elastic. The complexion is usually fresh and rather fair, the hair straight, and brown or dark, not so often black or fair; red and yellow seem to be rare; the eyes are usually hazel or greenish-brown, but blue and green are not uncommon. The head is mesokephalic, the upper part of the occiput prominent, and the greatest apparent height of the head somewhat far back, owing partly to the habitual attitude; forehead low and vertical, narrow in comparison with the head, but broad compared with the lower part of the face;

the superciliary arches moderately pronounced, the globella less so; the nose long and salient, the root not very deeply set; eyes lively, rather small in length, not prominent, rather far apart; a hollow beneath the orbit; the chin long, rounded but narrow, not very prominent, mandible narrow, often prominent beyond the maxilla, the angle with little salience downwards or forwards; the distance from the eye to the alæ nasi great, thence to the mouth short, and to the chin long.

Such is the portrait of the typical Basque, the correctness of which is amply avouched by many photographs; still, as Aranzadi acknowledges, it is not usual to meet with a man who answers to the type in every point.

Noticing that the curve of kephalic index was distinctly bicipital, with culminations about 76-77 and 80-81, he set himself to disentangle the remains of the constituent types. This his various measurements enabled him to do with a great appearance of probability, by showing that certain special varieties of feature, for example, blue eyes, were much more frequently associated with certain others, such as a long narrow nose and tall stature, than they should be by the doctrine of chances

Ultimately he dissected out three types.

First, one with green or greenish-brown eyes, set wide apart, brown hair, short stature, a broad head, a narrow nose and mandible, and a considerable facial angle.

Second, one with hazel or brown eyes, not wide apart, dark hair, middle stature, a long narrow head, a broad and concave nose, a broad mandible, a medium or small facial angle.

Third, and least numerous, one with small blue eyes, not wide apart, light brown hair, tall stature, a long narrow head, a long and straight nose and long narrow face, a rather narrow mandible (not so markedly narrow, however, as in the first type, which has a broader zygoma), a small mouth, and a small facial angle. The admixture of this type would appear to have been subsequent to the union of the other two.

The evidence for the existence of these three types is

far from weak, though one could have wished that the numerical basis had been larger, or that a control experiment had been worked out in another district, or by another hand. The great difficulty with me is that of identifying the first two types with any known races.

The first or brachykephalic one should, one would think, be of Keltic or Ligurian origin; but the broad head, supposed to have been brought into Spain by the Kelts, seems, on Aranzadi and Hoyos-Sáinz's own showing, to be accompanied by a broad nose, though in some other countries such is not the case. If it has here some points of resemblance to a Turanian or Mongoloid form, one need not wonder; that form is recognised by many as underlying the dark Kelts in France; Renan and De Labourdonnais see it in Bretagne, as I do, and others in the Morvan and elsewhere; but in these cases the nose is broad.

The second type may be Atlantic or Iberian, or Berber, or allied to the Cro-magnon. The relation of these *inter se* has not yet been cleared up; some of them may have been broad-nosed, but the true Iberian probably was not. Lagneau, the most learned and weighty authority from the historical point of view, would dissociate the Iberians from the other races just named.

The third type is the familiar blond long-head beyond doubt, the Aryan of some folk, the northern barbarian of others. In Guipuzcoa, Aranzadi shows it has some features common to the outcast Cagots, but in other respects it distinctly diverges. It abounds, apparently, especially in the north-east part, near the frontier passes, for some historical but forgotten reason doubtless.

Unfortunately we have no statistics of colour except these of Aranzadi's, which include a very limited area. Tubino and others tell us that the blond complexion is common in Asturias, Galicia, and parts of Old Castile, as well as in the Basque provinces; and Hoyos-Sáinz finds the long-headed blond type in Western Galicia. Tubino says the only part of the south where light hair is common, so far as he knows, is the mountainous region about Ronda, and Olóriz's figures exhibit this as an island of comparative

dolichokephaly. We know that there were blonds among the Turdetani, perhaps the rearguard of the migration of the Tamahu to North Africa.

Olóriz, allowing with Lagneau that the Atlantic and Iberian races were twain, that the latter were the later comers, and occupied the east of Spain, thinks with him that the Ligurians made partial settlements, and ascribes to them the comparative brachykephaly of Balaguer in Cataluña (80·0), and to some extent of Lower Andalusia. He thinks the Kelts broke in through the western extremity of the Pyrenees chiefly; that they could not expel the Iberians from Aragon and Soria, but pushed southwards and westwards, occupying the mountain chain of Cantabria, breaking through the Carpetanian Mountains about Segovia, settling thickly on the Tagus, and ultimately in Andalusia, the goal of all invaders. He does not think any great ethnical changes have occurred since the Roman period.

Aranzadi and his colleague would estimate such changes at a somewhat higher value. Attaching very great importance to the nasal index, and finding almost everywhere, as does also Olóriz, indications in the curve of head breadth of the presence in less force of the brachykephalic element which is potent in Cantabria and Toledo, and Cadiz, they are constrained to make a fourfold classification, into dolicholepto, dolichoplaty, brachylepto and brachyplaty. The Cantabrian and Oretanian (Toledo) regions are placed in the last, Lower Andalusia in the second class. The data, however, are as yet hardly sufficient for this arrangement. The authors, with a good deal of probability, think that the Arabs among the Moslem invaders reinforced the dolicholepto element of the population (which already included the Jews), the Berbers the dolichoplaty, which, as it is found even in Guipuzcoa,[1] which was never conquered by the Moors, must have existed already.[2] But, after all, im-

[1] And in the caves of Gibraltar.

[2] One witness to the large number of the Moslem invaders is the fact that they prevailed to change the names of several large rivers. There are five contiguous cantons in the provinces of Granada and Almeria, which form a dolichous patch on Olóriz's map, their average index being 76·7

portant as is the nasal form, we have as yet too little accurate knowledge about it. The Guanches were leptorrhine, like the Homme-Mort type in France.

The authors incline to attribute the potency of the blond dolicholepto type in South-west Galicia to the Suevian colony, which had a long occupation. Perhaps they only reinforced a type already planted there by some kindred of the Tamahu, or by the ruling caste of some Gallokeltic tribe. The Irish Milesians, if they came from Spain, as I believe they did, came "white of skin, and brown of hair," according to Mac Firbis.

BIBLIOGRAPHY.

LAGNEAU. *Les Ligures, memoire comm. a l'Institut.* Paris, 1876.

LAGNEAU. Des Anciens Peuples de l'Hispanie. *Comptes Rendus de l'Académie,* 1881.

LAGNEAU. Ethnologie de la Peninsule du S.W. *Mem. Soc. Anthr.*, 2nd serie, t. ii., 1882.

TUBINO. Rechs. d'Anthropologie Sociale. *Revue d'Anthrope.*, vol. vi., 1877.

DE ARANZADI y UNAMUNO. *El Pueblo Euskalduna.* San Sebastian, 1889.

DE HOYOS-SÁINZ and DE ARANZADI. *Un Avance a la Antropologia de España.* Madrid, 1892.

OLÓRIZ. *Distribucion Geografica del Indice Cefalico en España.* Madrid, 1894.

JOHN BEDDOE.

only. Now these are precisely the Alpuxarra Mountains, where the Moors held out longest against Ferdinand and Isabella, and the chief theatre of the Morisco war in a later age.

ON METAMORPHOSIS IN PLANTS.

EVERY one who is specially concerned with vegetable morphology at once recognises the important influence which the doctrine of metamorphosis exercises on this branch of botany.

By the metamorphosis, we express the perception that the marvellous variety in external form exhibited by plants is to be traced back to the diverse modifications of a very few fundamental types. Furthermore, it may also be pointed out that the study of metamorphosis is very closely bound up with that of adaptation, and indeed one may almost assert that a metamorphosed structure owes its existence to an adaptive modification of a relatively primitive type form. But as adaptations exhibiting identical characters may originate from different morphological sources, we realise that the function of an organ need not necessarily afford any clue to its real morphological nature. In dealing with the progress which has been made in this field of inquiry within recent years, it is not my intention merely to discuss a number of particular instances in which perchance the morphological characters have become more clearly apprehended than formerly. What we are here concerned with is rather the larger question of the *general* aspect of the subject before us.

And foremost amongst recent advances must be reckoned that change of front which has led to an abandoning of the attempt to refer to the reproductive organs, in the higher plants, as if they were actually modified vegetative structures. Formerly much controversy turned on the question as to whether a sporangium or an ovule should be considered as a modified hair (Trichome), or whether it should rank as a foliar (Phyllome) or axial (Caulome) structure. At the present time it is regarded as immaterial whether it corresponds in position with any one of these organs; its essential importance is discerned in its spore-producing character, that is, ultimately, its functional property. And spore-

bearing structures probably already existed as such (phylogenetically speaking) long before the vegetative structure on which they are now borne had reached its present degree of differentiation. Later on it will be made clear how it became customary to disregard the function of any plant-member, and to consider its position and mode of origin as alone of essential importance; for the present it will suffice to state that most investigators are agreed that the vegetative and the reproductive organs belong to distinct categories.

As regards the vegetative organs, it is admittedly difficult to fix upon any universally valid mark by which it would be possible to definitely differentiate, *e.g.*, all stems from leaves, or even stems from roots. But this does commonly become possible when we limit our consideration to single groups of tolerably closely allied forms, and this is sufficient for the purpose of the metamorphosis question. Nevertheless it must be admitted that families of plants exist in which the limits, for example, between stem and leaf (Caulome and Phyllome) become completely obliterated. An instance of this is seen in the plants composing the natural order Lentibularieæ, and especially in the species of *Utricularia*. The floating, free-growing lateral shoots which branch and bear the inflorescences of our common Utricularias, as regards their "morphological value," are leaves; and the same is true of the creeping, branched shoots of the terrestrial species. But these "leaves" also possess other characters usually attributed to stems. The line of demarcation between stem and leaf here completely disappears. In the more primitive species of this genus the leaves, however, may still perhaps be recognised by their apical growth. Similar remarkable cases of transformation are to be met with in roots, and to these we shall revert later on.

The question which immediately meets us is this: What significance are we really to attach to metamorphosis? Many answers have been given to this much-discussed question, and many have written on the subject without having, in the first instance, really formed any clear ideas

for themselves. I do not propose, however, to enter into the earlier history of this matter, as I have already treated of it elsewhere.[1]

Since the introduction by Wolff, R. Brown and Schleiden of embryological study into botany, the conceptions as to the meaning of metamorphosis have become limited to two in number. First, there is the theory of differentiation, and, secondly, that of an actual metamorphosis. According to the first view the embryonic tissue at the growing apex gives rise to *indifferent* structures which can develop in a variety of ways in accordance with the requirements of the plant as a whole, whilst, according to the latter, a *real* change takes place of the rudiment (*anlage*) of one particular organ into another organ, *e.g.*, that of a foliage leaf into a scale leaf, or that of a stamen into a petal. Before inquiring into the grounds on which these rival views are based I shall cite a passage from one of the most recent text-books of botany. Frank,[2] the author of the work in question, in speaking of the leaf, puts forward the following statement:[3] "When the rudiment (*anlage*) of a leaf, or leaf segment, which normally would develop into a foliage leaf is compelled in response to other conditions to develop as a scale leaf or as a sporophyll, all that has happened is this, that a leaf *rudiment* (*anlage*) may develop into either the one or the other organ; this, however, is not a real transformation of one organ into another one, for it is always the case that the rudiments (*anlagen*) which are capable of going through *various* developments (such as will ultimately result in the production of a foliage leaf, *or* a scale leaf, *or* a foliage leaf) are composed of undifferentiated embryonic tissue; when, however, they have assumed the form of any *one* of these structures they are incapable of changing into any of the others". The opinions just quoted are not new; indeed they represent

[1] Vergleichende Entwicklungsgeschichte d. Pflanzenorgane. *Schenk Handbuch d. Botanik*, iii., 1.

[2] *Lehrbuch d. Botanik nach dem gegenwärtige stande d. Wissenschaft.*

[3] Specially directed against my own view of the meaning of metamorphosis.

a widely spread view of the matter. But not only is the argument fallacious, but it is entirely opposed to certain experimental facts. In the first place it is illogical to speak of an undifferentiated organ or rudiment of an organ. One would have to admit the real existence of this abstract idea of a "leaf". But it is obvious that no general conception has a concrete existence. In dealing with abstract notions, we recede from the actually existent in proportion as our abstraction becomes more generalised. That which the organs, which we term respectively foliage, scale, or sporangium-bearing leaves, have in common must thus depend on something else than a general conception. And in this case are the rudiments (*anlagen*) really undifferentiated or not?

The rudiments of a leaf and of a lateral branch are commonly recognisable owing to their position long before they are otherwise distinguishable. No case is known in which a protuberance, which from its position should develop into a leaf, has become changed into a shoot, nor is any example of the converse forthcoming. The instance of Utricularia cited above clearly shows, however, that the lines of demarcation between the two series of organs as distinct categories may vanish. One is therefore compelled to admit that a leaf and shoot rudiment are normally different *ab initio*. Consequently it only becomes possible to assume the indifferent nature of the rudiments in the *same* category as, *e.g.*, in leaves and their various forms. But in this we are dealing, as has already been pointed out, with pure abstractions. Direct observation, furthermore, proves that the form of an organ may change during its development; that it may ultimately become something different from that which it at first promised to be. Thus the leaf-blade of a scale leaf aborts, the leaf-base enlarges. The organ is no longer undifferentiated; this is already proved by the abortion of the one portion of it. We are entitled to speak of a functional change in this case, seeing that the rudiment of a definite structure (a foliage leaf) has here become functionally and structurally altered into a scale leaf. This illustrates one of the most frequent cases

of metamorphosis which, according to one definition, is always associated with an alteration of function. But still more evident is the conclusion to be drawn from those instances where two functions gradually succeed each other. A few examples will make this clear.

The foliage leaf of *Lilium candidum*, and amongst Dicotyledons, that of some species of *Dicentra*, at first discharge the functions of any ordinary foliage leaf; subsequently its lower portion swells up to contain reserve materials of food, while the more apical region dies down. Here then is a real transformation of a part of the leaf which alters its form together with its function. An analogous case is that of a climbing plant, *Quisqualis chinensis*. The petioles of perfectly normal foliar organs change their form, and become converted into hooks, by means of which the *Quisqualis* shoot is enabled to climb.[1] In some species of *Astragalus* and *Caragana* the midribs of the pinnate leaves persist, and, after the leaflets have been cast off, become converted to thorns. Many other instances could be quoted in which it would be impossible to deny that a *real* metamorphosis takes place; an organ specialised in one direction assumes the function, and concomitantly the form, of another. If we now suppose that the leaflets of an *Astragalus* were to fall off before they unfolded, but after their rudiments had appeared, would not this be still a case of real metamorphosis? What we term the mature condition of a leaf is only the final term of a developmental series of phenomena, and it would be erroneous to conclude that the final form is the solely important character of any structure; each of the successive incidents in its development is rather to be regarded as the result of the preceding ones; to designate a leaf rudiment (*anlage*) as a mere mass of "indifferent" cells is equivalent to denying that there is any cause which effects its gradual ontogenetic development. It is the *entire* course of its progress to maturity which supplies the essential characteristic features

[1] They change into hard woody structures, whilst the leaf-blade is thrown off: at first, however, they function as ordinary leaf-stalks.

to the organ. The actual ontogeny through which it passes depends upon its own material peculiarities, and this is equally true whether we seek to explain the facts in the light of the chemical composition or in the configuration of the material particles. Thus the leaf does not at first acquire its identity at the final stage of its development, for its whole course of growth is normally already predestined. The operation of new forces acting at a suitable time can indeed materially influence the actual form which is finally assumed, and, speaking generally, the extent of the deviation effected approximately depends upon the time of the first effective appearance of this disturbing influence.

Instances analogous to those already mentioned might be cited in the case of other organs, but for the present it will be sufficient to give an experimental proof of the existence of a real metamorphosis.

This experiment is performed frequently enough in nature, in the case of many of those phenomena which we term monstrosities. In this place, however, we are more especially concerned with those of which we know the causal agency. When the flower buds of *Knautia arvensis* are attacked by the fungus known as *Peronospora violacea* the staminal rudiments develop as petals. The former are by no means "indifferent" structures. The series of changes which would normally result in the formation of typical stamens may have already begun, but the course of development becomes modified by the action of the fungus. Similarly it was shown by Peyritsch that from the rudiments, perfectly normal rudiments (*anlage*) of ordinary single flowers, double blossoms may be produced by the action of insects. My own researches have been especially directed to the foliage leaf. I have succeeded in proving that the history of development shows that the bud-scales of our woody plants arise from ordinary leaf rudiments (*laubblatt anlagen*), and that the deviation from the form of a typical foliage leaf can be prevented by a very simple operation; so that the structure which, ordinarily, would have assumed the arrested form (scale leaf) will, instead, become a green leaf. And the same is true, *mutatis mutandis*, for the

sporophylls of ferns, in which the foliage leaves often are strikingly distinct from those which bear the sporangia. It is possible, experimentally, to compel the sporophylls of *Onoclea Struthiopteris* to develop as foliage leaves. The investigation of the young stages shows that up to a certain period of their life the two structures are quite indistinguishable, and it is only subsequently, and owing to the nutritional changes connected with the appearance of their sporagia, that the sporophylls depart from the form exhibited by the rest of the leaves of the plant. This correlation between sporangial formation and the shape of the leaf (which, however, is not equally distinct in all ferns) is also admitted by Bower; he believes, however, on phylogenetic grounds, that the conclusion here arrived at should be reversed, and that the foliage leaves ought rather to be regarded as metamorphosed sterile sporophylls. But in my opinion, Botany, as a science, is more concerned with facts of experience than with phylogenetic theories. Experience shows that in all ferns foliage leaves are the first to appear, and that these may subsequently become altered, so as to form sporophylls. As to what occurred in prehistoric ages, we require far more convincing proofs than are afforded by the materials at present within our reach, before we can speak with any assurance. Meanwhile, one may say with Faust: —

> Mein Freund, die Zeiten der Vergangenheit
> Sind uns ein Buch mit sieben Siegeln.

And I, for my part, prefer to abide by the evidence actually before us.

As regards the shoot, there is also experimental evidence forthcoming. Some of this was furnished by T. A. Knight,[1] whose services to morphology have often been insufficiently recognised.

Knight experimented, especially with the potato. The subterranean tubers of this plant are, as is well known, merely altered shoots. Knight, however, succeeded in

[1] *Cf.* A Selection from the Physiological and Horticultural Papers by Thomas Andrew Knight, London, 1841. Also, Goebel, "Zur geschichte unserer Kentniss d. correlations-vorgänge," *Flora*, 1893, p. 38 ff.

inducing the formation of tubers on the aërial portions of the plant, where, in the ordinary course, only leaf buds or flowers would have occurred. He was also able to compel the underground stems (normally tuberous) to develop as leafy shoots above the ground. According to his view, the formation of tubers depends on this, "that a fluid descends from the leaves and stems to form the tuberous roots of this plant". This "fluid" can, however, be compelled to travel[1] in the parts above ground, and then the leafy buds become changed to tubers. It seems clear, then, that Knight recognised a *real* metamorphosis as having here taken place.

Moreover, it is possible to induce, in the same manner, shoots which would ordinarily develop as thorns to grow into leafy shoots. Lothelier[2] has lately stated that this may occur in a manner differing from that above indicated. It has already been observed that metamorphosis, regarded from a morphological point of view, consists in this,—that the rudiment of an organ is arrested at a particular period of its growth, and that it thenceforth is changed, as regards its further development. Now, the thorns of *Lycium barbarum*, *Ulex europæus*, *Genista anglica*, etc., are merely leafy shoots which have been arrested in their further development and have undergone sclerosis. If, however, the plants be grown in a very damp atmosphere, the shoots, which otherwise would have formed thorns, continue to grow, according to Lothelier, and elongate into leafy branches. In other words, in the latter case the rudiments of these structures do not remain stationary on reaching a certain stage of development, but continue to grow in a normal fashion, since the external factor which here effected the

[1] I cannot discover whether Knight succeeded in causing the *apex* of an aërial shoot (which should normally have produced flowers) to penetrate into the ground as a rhizome. I was successful in this during last summer with *Circæa*. An account of these experiments, however, will be given more fully elsewhere.

[2] Lothelier, "Influence de l'état hygrometrique sur les tiges et les feuilles des plantes à piquants" (Thèses presentées à la Faculté des Sciences de Paris, 1893).—Whilst repeating these researches, I have *not* been able to obtain the results mentioned by Lothelier, at least not in the same measure as he describes.

alteration (to thorns) is now absent. Just in the same way the bud of an oak shoot does not form a gall, except when stimulated by the Cynips.

It is needless to multiply instances further. I would only remark that there exists between metamorphosis and the stunted condition of an organ this much in common, namely, that in both cases an arrested development of the organ occurs. Organs which are gradually disappearing may be recognised by the fact that they may, actually, attain varied degrees of perfection, being sometimes more sometimes less highly differentiated.

Besides the results of experiments, the transitional forms which occur between one structure and another afford additional proof of the existence of a real metamorphosis. This line of argument was exclusively followed until attention became directed to the facts of actual development, and it was especially associated with the study of monstrosities. Teratology cannot, however, be considered here, as it would require a complete article to itself. It must thus suffice to mention a few instances taken from normal developments.

The inflorescences of *Nidularium splendens*, a Bromeliad, are surrounded by a number of beautiful red leaves which exhibit differences of form amongst themselves. The lowest resemble the ordinary leaves of the plant, save only for the red colour which occurs in their basal regions. In the successive leaves the coloured area steadily increases, until the entire organ is coloured from base to apex ; that is, the colour-alteration affects the leaves at different times in their history ; first in the later period, and gradually at ever earlier stages, and it is to this fact that the existence of the various transitional forms between foliage leaves and bracteoles is due. In other Bromeliads, for example in *Bilbergia*, the transition is very abrupt. Instances of a similar character are common enough amongst dicotyledons, and it will be sufficient to mention here the cases of *Eranthemum nervosum* and *Astrantia major*.[1]

[1] Other instances are quoted in my book, Vergleich. Entwickelungsgesch. d. pflanzen organe.

Sparmannia africana may be cited as an example derived from floral structures. The flower possesses numerous stamens, of which the outer ones are modified so as to form conspicuous bodies, which attract attention to the blossoms, and are concerned with the secretion of nectar. Accordingly, the filaments are not smooth, as is usually the case, but are bright yellow in colour, and they secrete, especially in sunshine, drops of fluid. These stamens, the pollen sacs of which are either entirely absent or are merely vestigial, are connected with the normal stamens by a complete series of intermediate forms. *Anemone pulsatilla*, in which the outer stamens are also converted into nectaries, similarly exhibits all the transitional stages to the perfect organ. I think these facts only become intelligible on the view that the modification of the stamen-rudiment may set in at different periods, and thus the formation of intermediate forms is just what would have been anticipated.

The intervening stages which, in the examples already quoted, can be recognised by the unaided eye, may also be definitely followed out under the microscope; it will be better, however, to consider this side of the subject in connection with the *root*, as the conditions are simpler than in the case of the leaf.

Every one understands a "typical" root as an organ possessed of definite peculiarities of form and anatomical structure. Now, these peculiarities are very intimately connected with, and depend upon, the functions which the root has to perform; and, indeed, only on this view do they become intelligible. There are, however, some roots which, though at first behaving in all respects as normal organs, subsequently cast off their calyptra, and henceforth assume the mode of growth characteristic of shoots. Until recently, only two cases (*Neottia nidus-avis* and *Anthurium longifolim*) of this nature were known, but others have now been traced in the case of several ferns (*e.g.*, species of *Platycerium* and in *Asplenium esculentum*). No one can refuse to admit that a *real* metamorphosis has occurred here, and one which is associated with profound anatomical

changes. The same thing takes place when a root is converted into a thorn (as in *Myrmecodia echinata*, or in *Acanthorhiza aculeata*), or into a storage receptacle for reserve food. We see in all these, how the root cap (calyptra) becomes lost, and the internal structure undergoes modification, in accordance with the new functions which the root is now called on to discharge. But these changes may also be indicated before the root has actually begun to exercise its normal office. Perhaps the most striking examples of this are to be found amongst those roots which come to serve as organs of carbon-assimilation. This happens in several epiphytic orchids, as well as in that remarkable aquatic order, the Podostemaceæ. In certain orchids, indeed, the roots are the sole assimilatory organs, for the leaves have become degraded to mere colourless scales (*Angræcum funale*, *Taeniophyllum sp.*). And not only do the roots become green, they may even assume the dorsiventral structure of leaves, becoming flattened out on the illuminated surface. I have observed, however, that in the case of *Taeniophyllum*, the growing apex is still radial, the flattened form being subsequently assumed during development.

Still more strikingly leaf-like are the roots of *Dicræa algiformis*, a plant which was carefully studied by Warming. In every one of these instances, observation shows that it is by no means in an "indifferent" mass of tissue, but in one which is already clearly recognisable as appertaining to a root, that the alterations of function and form have taken place.

We may now turn and consider the leaf, and follow the development of certain foliar tendrils, from the special point of view immediately before us. And here we are at once confronted by the question, as to whether the history of development will enable us to arrive at a satisfactory conclusion as to the foliar nature of these structures. This question may at once be answered in the affirmative. Indeed, microscopic investigation is frequently unnecessary, for the study of the seedling often affords evidence sufficient for us. Thus, amongst the Fumariaceæ, in *Corydalis*, for example, all intermediate stages may be observed. The

first leaves, as is usual, do not twine, but in the succeeding ones the stalks of the leaflets gradually elongate, while the blade-portion concomitantly suffers reduction until typical tendrils are produced, and the same process obtains in many other plants. To quote one more example, the tendrils to *Cobæa scandens* owe their existence to luxuriant branching of the terminal portion of the leaf; the hooks of the tendril represent very much modified leaflets, while the arms are derived from the stalks of the pinnæ. Occasionally one meets also with transitional forms which are intermediate between tendrils and leaflets, and which one can easily recognise as such without the aid of the microscope. In other cases, the *entire* leaf rudiment (*Blattanlage*), and not merely the leaf stalk, may develop into a tendril. Thus in the first-formed tendrils of *Benincasa cerifera*, a cucurbitaceous plant, it is easy to recognise the rudiments of a leaf-blade, but this becomes increasingly difficult in the tendrils which are formed later on.

As regards the Phyllodes of the Australian Acacias, they are often spoken of as flattened petioles, the leaf-blades of which have completely vanished. But, after what has been already said, it is obvious that this view is untenable, and as a matter of fact, the blade of the leaf is clearly enough visible, only it remains very small. This explains how it may happen that in some species the phyllodes are accompanied by fully developed foliage leaves, and observations on the seedling plants also indicate plainly the steps by which a *real* metamorphosis has been effected. Additional examples might easily be quoted, but they would only illustrate the same point, namely, that metamorphosis is an *actual objective fact;* that the rudiment which is normally destined to develop into a definite organ may change its ordinary proportions during development, and thus give rise to another organ, and that this change of form is constantly associated with an alteration of function. Thus we have come to limit the conception of metamorphosis, so that it only applies to the results produced by the process of division of labour amongst homologous members. We recognise, in dealing with the homologous members of a branch system,

that the separate individuals may differ amongst themselves both in form and in function, just as in a mass of roots the lateral fibres differ to some extent from the positively geotropic primary root; or again, in a shoot, such as that of Equisetum, the lateral branches which perform the task of assimilation do not precisely resemble the axis from which they spring.

It is true that division of labour does not necessarily involve metamorphosis, but it may certainly be regarded as a condition of its manifestation. Hence, not morphology but physiological requirement is the main factor, teleologically speaking, in effecting structural differentiation, and the particular organ which will undergo the modification will be that which is most susceptible to a change in the desirable direction. This consideration serves to account for the apparently capricious character which modifications sometimes exhibit. Why should the leaf stalk, for example, become flattened out and replace the blade in *Oxalis bupleurifolia*? Assuredly because tne latter structure was incapable of fulfilling the conditions imposed by the environment. An analogous case is that of *Genista sagittalis*, a plant which inhabits arid districts, and thus requires to limit the area of its transpiring leaf surface. On the other hand, however, its stem has become furnished with broad, leathery wings, and these serve to increase the assimilating surface. It is obvious that in this case also the leaves were unable to satisfy the requirements of the plant's surroundings, and hence the wings arose as their substitutes. And the experience of plant cultivation still more clearly demonstrates the conclusion that the various organs of different plants may exhibit the power of adaptation in any given direction in widely differing degree.

K. GOEBEL.

THE COAGULATION OF THE BLOOD.

SUPPLEMENTARY NOTE.

SINCE the paper with the above title was written for the January number of this volume, an important paper on the subject by L. Lilienfeld[1] has appeared. The paper is a long one, as it includes a historical account of this vexed question, and also contains in full the experiments on which the author has based several previous preliminary communications. The main argument may be stated as follows: The active agent in coagulation is regarded as a nucleo-albuminous substance which is called nucleo-histon. It is considered to originate from the leucocytes, especially from their nuclei and from platelets. The nuclein of the nucleo-histon hastens, while the histon moiety hinders coagulation; the separation into nuclein and histon may be effected by lime water or baryta water. Nuclein alone, and calcium chloride alone, do not cause coagulation in solutions of Hammarsten's fibrinogen; but if acetic acid is added to a solution of fibrinogen, a substance is precipitated therefrom which is coagulated by nuclein with calcium salts, or by calcium salts alone. This substance is termed thrombosin; and fibrin is regarded as a calcium compound of thrombosin. The constituent of fibrinogen, which remains in solution when thrombosin is precipitated by acetic acid, is like peptone in some of its properties, particularly in its hindering influence on coagulation. Nuclein, or rather nucleic acid, acts just like acetic acid in precipitating free thrombosin from fibrinogen (not a nucleic acid compound of thrombosin). This is stated to hold good both for intravascular and extravascular blood; nucleic acid first splits up the fibrinogen molecule, and then one of its components, thrombosin, unites with a calcium salt to form fibrin. The fibrin-ferment is described as a globulin, which is a product, and not the cause of coagulation.

[1] *Zeit. Physiol. Chem.*, vol. xx., pp. 89-163, 1894.

Such a theory is extremely interesting, and no doubt will stimulate fresh work by which the data on which it rests will be thoroughly tested.[1]

I may take this opportunity of alluding rather more fully to the work of Dr. J. Reynolds Green, who, I learn from a private letter, considers that in my former paper I did not sufficiently emphasise the fact that the credit of the discovery of the importance of calcium salts in coagulation should be shared between himself and Arthus and Pagés. It was certainly not my intention to minimise the importance of Green's work, but exigencies of space compelled me to allude to it rather briefly. Dr. Green clearly pointed out that calcium salts were necessary for coagulation, for if they were removed by dialysis, or by the action of barium chloride, no clotting took place. He, moreover, goes further than Arthus and Pagés in considering that the salt of calcium required is the sulphate, for this salt is the most active in setting up the clotting, and fibrin itself contains this substance (Green and Sheridan Lea).

W. D. HALLIBURTON.

[1] Indeed such work has already commenced. Prof. E. A. Schäfer, F.R.S., has communicated to the Physiological Society (*Proc. Phys. Soc.*, March 16, 1895) a preliminary notice of some experiments he has performed. He is unable to accept Lilienfeld's distinction between fibrinogen and fibrin, and also doubts Lilienfeld's conclusion that the nucleo-albumin does not enter into the composition of fibrin.

THE ARRANGEMENT OF THE ATOMS IN A CRYSTAL.

A GEOMETRICAL problem which has long exercised the minds of crystallographers has recently been solved in a complete and satisfactory manner. The problem is: To ascertain the nature of the symmetrical repetition in space of its ultimate parts which confers on matter the symmetry shown by crystals.

Like most other important contributions to exact scientific knowledge the solution is the result of investigations carried on by several different workers in different countries, and it has only been brought to its present finality by slow degrees. In a previous article it was shown how the researches of Sohncke in particular supplied the principle by the extension of which the present successful solution has been obtained. A crystal was conceived by Sohncke as represented by a system of points so arranged in space that its aspect is precisely the same viewed from every one of the points; or the property may be otherwise expressed by stating that there are certain movements of translation, or rotation or screw-motion, which can be imparted to the system without changing its aspect after they are completed; this was Sohncke's original way of expressing the fact that a crystal is homogeneous. It was explained in the previous article that the structural theory of Schönflies and Fedorow, which furnishes a complete solution of the problem, differs from that of Sohncke by admitting the principle of reflection across a plane, or inversion about a centre, or a combination of the two, as a further possible mode of repetition applicable to a system without changing its aspect.

Since that article was written Barlow has published an important paper (*Zeitschrift für Krystallographie*, vol. xxiii., p. 1) in which he applies the principle of reflection or inversion to structures based upon Sohncke's regular systems of points. In this paper, however, the problem is not

stated as relating to points or molecules, or anything in particular, but as one dealing with the arrangement of the similar parts in a homogeneous rigid structure of any sort. The author defines a homogeneous rigid structure as an arrangement of material of any quality, and of constant form which is uniformly repeated over its whole extent.

The definition is more fully expressed as follows: A homogeneous structure is one in which, if it be regarded as without boundary, to every point correspond other exactly similar or homologous points which are uniformly distributed in space; it is therefore a property of the structure that each infinite system obtained by selecting all similar points is a regular system as defined by Sohncke; that is to say, round every point of such a system the arrangement of the remainder is the same as round every other.

A pile of cannon balls is a homogeneous structure; so is a stack of cubes packed together closely in as regular a manner as possible. We will take the latter as an example of the manner in which Barlow's method may be applied.

Consider in such a structure the corner of any one cube; it is a point in which eight cubes meet: now take a point within the cube; the point may be anywhere, but for convenience we will suppose it near the corner. It will be found that there are two other points, at an equal distance from the same corner, from each of which the aspect of the whole structure is absolutely the same, and identical with the aspect from the first point chosen; further there are three more homologous points near each of the remaining corners of the same cube, making twenty-four in all, from each of which the aspect of the structure is still the same. It is easy to convince oneself of this by regarding the cubes as rooms with transparent walls, within one of which the observer is placed, and contemplates from it the remaining rooms, above, below and on all sides; he must of course be prepared to stand upon his head, or put himself into any required position, and take no account of the difference between upwards and downwards, north and south, etc.

All these points then, twenty-four in each cube, taken together throughout the whole structure, constitute one of

Sohncke's systems of points; we will call them the A system.

It might be supposed, at first sight, that there can be nothing of a right- or left-handed character about the perfectly symmetrical cubic structure here described. But it will be found, on consideration, that there are, further, three other points near each corner of the original cube, from each of which the aspect of the whole structure is exactly what it was before, with the exception that everything is inverted as though in a mirror; right hand has become left hand, and the observer is contemplating, as it were, the reflection of what was seen from the first position.

The system composed of all the latter points, twenty-four in each cube throughout the whole structure, is a second Sohncke system, B.

Next, suppose each cubic room to be similarly furnished; and let the articles of furniture be of such a shape and so placed with regard to the walls, floor and ceiling that the aspect of the room from each point of A is the same, while the aspect of it from any point of B is *not* the inverted image of the view from A. The whole set of rooms with its furniture is now a homogeneous structure, and will be found to have the symmetry of an asymmetric cubic crystal such as sal-ammoniac. Such a structure is not identical with its image. Again, let the articles be so placed and of such a nature that the aspect from each point B *is* the inverted image of the view from each point A; the structure is still a homogeneous one, but has now the complete symmetry of a cubic crystal such as fluor, and is identical with its image.

Barlow regards this property of identity with its own image as the only property compatible with its homogeneity which can increase the symmetry of a homogeneous structure. He takes therefore as the basis of all conceivable homogeneous structures the sixty-five regular systems of points established by Sohncke; he inquires which of the sixty-five types of structure can possess the additional property of identity with their own images, and in how many ways they may do so; and he finally arrives at

a result practically identical with that of Fedorow and Schönflies; namely, that there are 229 possible varieties of homogeneous structures, and that these present exactly the same thirty-two types of symmetry which are to be found in crystals, and no others.

The conclusion of the whole matter is, then, that the peculiar symmetry of crystals can be fully explained as resulting from their homogeneity. That they are homogeneous structures we are bound to believe from the fact that, so far as our observation goes, a crystal is the same at every point within its mass; it is impossible to distinguish one internal part from another; and this character can scarcely be attributed to anything else than the juxtaposition of identical parts or particles.

The geometrical principle of homogeneity with its resulting symmetry is one of the very greatest importance, and must be taken into account in any theory of the constitution of solid matter; no theory of crystalline structure which ignores it deserves serious consideration, and any theory which is antagonistic to it may be at once dismissed.

Lord Kelvin, in his interesting Boyle Lecture, delivered at Oxford in 1893, has shown how the same problem may be regarded in three different ways. In the first place, it is possible to consider, as has been done above, the regular arrangement of a number of identical figures or units of any sort; secondly, one may imagine them to be so arranged that they are all in contact, the problem then becomes one concerning the close packing of bodies of a given shape (for example, a pile of cannon balls); or, thirdly, these may be imagined to increase in size until they are all in *complete* contact over their whole surface, the problem then becomes one of the partitioning of space into cells.

But, in any case, corresponding parts of the structure, that is to say, parts which are either identical or inverted (in the sense described above), must conform to one of the 229 types now established, and the nature of such structures is limited by this condition.

ATTEMPTS TO ASCERTAIN THE STRUCTURE OF KNOWN CRYSTALS.

The next problem is to determine which of these types are physically possible and can be used to explain the structure of known substances.

It is customary to suppose that when a substance crystallises, the gaseous molecules, or the liquid molecules, or the free ions, coalesce to form complex groups to which the name of crystal molecules is given, and the regular arrangement of these clusters is supposed to constitute the structure of the crystal. Further, it is usually believed that the arrangement of the crystal molecules is determined by their inherent symmetry, and the symmetry of each crystal molecule is unhesitatingly ascribed to the arrangement of the atoms within it.

It is obvious that with such a string of assumptions as are here necessary the reasoning may be to the last degree fallacious. Even supposing that we are justified in regarding the chemical molecule as having a configuration due to the arrangement of rigid parts, it is difficult to see why the symmetry of the crystal molecule must be the same as that of the crystal; and if the chemical molecules of which it consists be differently orientated, it is still more difficult from the symmetry of the crystal to draw any conclusion regarding that of the chemical molecule.

Returning for a moment to the illustration of the cubical rooms employed above, it might be expected that if the nature of such a structure were under investigation the first step would be to ascertain the form of the rooms, and afterwards to determine the distribution of the articles which they contain. In the same way, although we do not know whether the crystal molecule (if it exist) is to be regarded as represented by the room itself or by any one of the symmetrically distributed articles, it might at any rate be expected that attempts would first be made to ascertain how the crystal particles are arranged, rather than to investigate their symmetry or that of the chemical molecule.

Curiously, however, although scarcely any successful progress has been made towards determining the arrangement of the molecules in any actual crystal, some very interesting attempts to determine the relative positions of the atoms within the molecule of certain crystals have recently been made, and deserve consideration as the first efforts to found such speculations upon observed facts.

APPLICATION OF THE LAW OF GLADSTONE.

Sollas (*Trans. Roy. Dublin Soc.*, v., p. 157) has attempted to interpret the law of Gladstone and Dale, applied to crystallised substances, as indicating the relative positions of the atoms.

This law, which states that $\frac{n-1}{d}$ is a constant (the *specific refractive energy*) for a given substance whether existing alone or combined with other substances, is intelligible in the case of liquids or gases, where n denotes the refractive index and d the density; the specific refractive energy of a compound can then be deduced from that of its components, for the refractive equivalent of the compound (*i.e.*, specific refractive energy multiplied by molecular weight) is the sum of the refractive equivalents of its components. Now, for crystals belonging to the cubic system, both n and d can be supposed to have the same meaning as in the case of liquids, for such crystals are isotropic; but in other substances the index of refraction varies with the direction of a ray of light within the crystal, and we cannot speak of *the* refractive index of the substance. Yet, curiously enough, it has been established as an empirical fact that the law holds perfectly for crystals if the *mean* refractive index be used in the formula.

Sollas then inquires what is the signification of the formula when the maximum and minimum indices are used respectively in place of the mean index. Thus sodium nitrate crystallises in the rhombohedral system, and has the two following principal indices:—

Ordinary refractive index = 1·586.
Extraordinary ,, = 1·336.

The salt crystallises in rhombohedra; the author assumes that "the molecule must conform to the symmetry of the rhombohedral system, and for this at least three sodium nitrate molecules must be conjoined, while to produce a rhombohedron six will be required". He supposes then that these six molecules are distributed into two groups, one consisting of oxygen, six atoms of which are vertically linked to a nitrogen atom, and the other of six rays of -O-N-O-Na, the chemical bonds of which lie more or less parallel to the equatorial plane. Rays of light travelling perpendicular to the optic axis determine the two principal indices of refraction; and the next supposition made is that the ordinary index relates to the horizontal group of atoms, and the extraordinary to those which are vertically linked; in other words, the ordinary index relates to $Na\,N_{\frac{3}{5}}\,O_2$ and the extraordinary index to $N_{\frac{2}{5}}\,O$. It will be observed that this introduces a fresh assumption, namely, that we are justified in assigning three-fifths of the molecular weight of the nitrogen to the equatorial members of the molecule, because only three out of the five bonds are horizontally linked.

Applying the formulæ

$$\frac{n_1 - 1}{k_1} = d_1$$

$$\frac{n_2 - 1}{k_2} = d_2$$

where n_1 is the ordinary, n_2 the extraordinary index,

k_1 is the refractive energy of $NaN_{\frac{3}{5}}\,O_2$

k_2 ,, ,, ,, $N_{\frac{2}{5}}\,O$

we get $d_1 = 2{\cdot}693$, $d_2 = 1{\cdot}474$.

What may be the physical meaning of these two quantities the author does not venture to suggest, but he next assumes that the molecular weight of the first atomic group divided by d_1 gives its volume v_1, and that the molecular weight of the second divided by d_2 gives similarly v_2, the volume of the group $N_{\frac{2}{5}}\,O$, and that the sum of these partial volumes is equal to that of the whole salt.

This is, in fact, very nearly the case, for $v_1 = 23{\cdot}54$, $v_2 = 14{\cdot}65$ and $v_1 + v_2 = 38{\cdot}19$, while the volume of the salt is $37{\cdot}85$.

Evidently there are many ways of arranging the atoms to accord with any required symmetry, and these will all give different values of d_1 and d_2; the only criterion which can be applied in order to discover which is to be adopted in any instance is the proposed equality between the sum of the partial volumes and that of the salt itself.

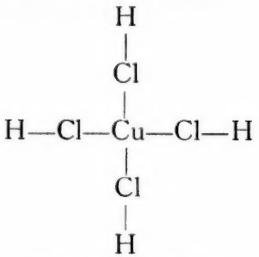
The insecure nature of the evidence is so conspicuous that it is scarcely necessary to point out how at every step in the reasoning a fresh assumption is introduced. We do not know in the least that three molecules are necessary for rhombohedral symmetry, or that the nitrogen atom is attached to five oxygen atoms, or that it is allowable to imagine the optical action of the nitrogen divisible into fifths, or that there is any such thing as vertical or horizontal linking, and least of all do we know the signification of the quantities d_1 and d_2 which are made to play the part of densities measured along a certain direction. Without other confirmatory evidence it is impossible to put much faith in the criterion by which the atomic arrangement is tested, or to say that the hypothesis possesses more than a certain plausibility.

But all this does not diminish the interest of these suggestive speculations or the extreme ingenuity with which they are brought forward by Sollas.

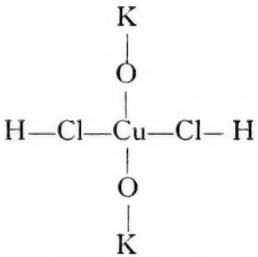
An additional piece of evidence in favour of the distribution which he suggests for sodium nitrate is the fact that in potassium nitrate the least index of refraction is almost identical with the lesser index of the sodium salt; hence it might be expected that this index is related to the radicle which is the same for both salts, namely, the nitric oxide; and such a view accords with the atomic distribution suggested above.

Another case in which somewhat stronger confirmatory evidence is adduced is that of the hydrated potassium copper chloride $Cu\,Cl_2,\ 2K\,Cl,\ 2H_2\,O$, which crystallises

in the tetragonal system. Here the atomic distribution adopted is as follows :—



Horizontal Plan.



Vertical Elevation.

This gives $v_1 + v_2 = 132{\cdot}9$; a number almost exactly equal to that obtained from the formula of the whole salt.

Now the crystals are markedly dichroic, the ordinary ray being green, and the extraordinary blue; it is suggested that this corresponds to the above arrangement; for the composition along the vertical line is $Cu_{\frac{1}{6}}$ O K or Cu O_3 ; and that along the horizontal line $Cu_{\frac{1}{6}}$ $Cl_{\frac{1}{2}}$ or Cu Cl_3. The former is more of a cupric nature than the latter, and the cupric salts are characteristically blue as contrasted with the green cuprous salts.

This suggestion of course opens up a most interesting series of questions as to the relative distribution of ferrous and ferric constituents in the pleochroic compounds of iron. But we must here leave the bold and ingenious speculations of Sollas in order to glance at the manner in which the same problem has been approached by other investigators.

DETERMINATION OF TOPIC AXES.

Muthmann (*Zeitschr. f. Krystallographie*, xxii., p. 497), and, independently (at the suggestion of Becke), Tutton (*Journ. Chemical Soc.*, lxv., p. 628), have endeavoured, by extremely accurate determinations of the angular relations and of the specific gravity of certain substances, to determine the relative distances of the crystal molecules from each other along different directions in a series of isomorphous salts. The argument is this : If A_1 B_1 C_1 be the

lengths of the sides of the elementary parallelepiped of the crystal (the unit cell described in a previous article), and V be its volume, and if $a\ b\ c$ be the crystallographic axes; then supposing (merely for illustration) this elementary cell to be rectangular and the axes to coincide in direction with its sides; $V = A_1\ B_1\ C_1 = \frac{p}{s}$, where p is the molecular weight, and s is the specific gravity of the salt. $A_1\ B_1\ C_1$ therefore represent the molecular intervals along the edges of the cell—and these edges may be taken for crystallographic axes.

Then $\frac{A_1}{a} = \frac{B_1}{b} = \frac{C_1}{c} = \mu$

$$V = abc\ \mu^3 \text{ and } \mu = \sqrt[3]{\frac{V}{abc}}.$$

From this it follows that

$$A_1 = \sqrt[3]{\frac{a^2V}{bc}};\ B_1 = \sqrt[3]{\frac{b^2V}{ca}};\ C_1 = \sqrt[3]{\frac{b^2V}{ab}}.$$

Similar formulæ involving one or more measured angles can be easily obtained when the cell is not rectangular, or where its sides do not coincide in direction with the crystallographic axes.

$A_1\ B_1\ C_1$ are called the topic axes of the crystal.

So far the topic axes present no advantage over the ordinary crystallographic axes, since they are merely numbers expressed in terms of an unknown unit. But the next step is to calculate the topic axes for a number of substances belonging to a strictly isomorphous series; Muthmann did this for the acid phosphates and arsenates of potassium and ammonium, and for certain alkaline permanganates; Tutton for the sulphates of potassium, rubidium and cæsium.

Let $A_1\ B_1\ C_1$, $A_2\ B_2\ C_2$, etc., be the topic axes for such a series. We may reasonably assume that the molecular arrangements of isomorphous crystals are similar, and, therefore, that they have the same elementary parallelepiped, which only varies slightly in its dimensions for the various members of the series. If this be so we are enabled to compare (for the first time) crystals of different substances.

Not only do A_1 B_1 C_1 give the relative molecular intervals along three directions in crystals of the first substance, and A_2 B_2 C_2 along three corresponding directions in the second, but the ratio $A_1 : A_2$ represents the relative molecular separation along corresponding directions in the two different substances. Hence in a series of isomorphous substances we are enabled to express the increased or diminished separation of the molecules along a given direction which results from the replacement of one element or radicle by another in the series.

Thus, as regards the first series examined by him, Muthmann found that in the tetragonal phosphates and arsenates the replacement of an atom of phosphorus by an atom of arsenic causes a separation of the molecules which is almost uniform in all directions; when, on the other hand, in either the phosphate or the arsenate the potassium atom is replaced by the ammonium radicle, the molecules are again separated, but now almost entirely in the direction of the principal axis.

From this interesting result he concludes that the metallic elements occupy such a position in the molecule that lines uniting them to the acid radicles are parallel to the principal axis.

With the further assumption that the symmetry of the crystal molecule is tetragonal like that of the crystal, and therefore that it must contain at least eight chemical molecules, Muthmann finally proposes a scheme of the structure of the crystal molecule, in which eight K O radicles are placed vertically above and below four pairs of P O $(O H)_2$ radicles arranged at the corners of a square.

In a similar manner Tutton proposes as the structure of the orthorhombic group of alkaline sulphates an arrangement of four chemical molecules, and suggests the relative positions of the metallic atoms.

It will be evident that in these speculations again we are confronted by a long series of assumptions; we do not know that the volume of the elementary parallelepiped is the equivalent volume of the substance; we have no convincing evidence to indicate the form of the elementary

parallelepiped; it is by no means certain that the crystallographic or topic axes represent molecular intervals; and there is no direct evidence to show that the symmetry of the crystal molecule (whatever that may be) is the same as that of the crystal itself.

ADDITIVE RELATIONS OF CHONDRODITE.

Another important contribution to the subject is one in which the crystals compared are not isomorphous, but bear an additive relation to one another; it is a case in which the substances differ not by the replacement of one element or radicle by another, but by the addition or subtraction of the same radicle.

By careful analysis of the minerals of the chondrodite group Penfield (*Amer. Journ. Science*, xlvii., p. 188) has proved that there are three compounds having the composition—

(A) $Mg_3[Mg(F.OH)]_2(SiO_4)_2$ No. of Mg atoms 5
(B) $Mg_5[Mg(F.OH)]_2(SiO_4)_3$,, ,, 7
(C) $Mg_7[Mg(F.OH)]_2(SiO_4)_4$,, ,, 9

Each of these is derived from the preceding one by the addition of the olivine radicle Mg_2SiO_4.

The three minerals have almost the same form, but differ only in the length of one crystallographic axis. The lengths of this axis in the three minerals are in the ratio 5 : 7 : 9. Thus the addition of the olivine radicle Mg_2SiO_4 exerts a so-called "morphotropic" action along one definite axis, and causes a certain constant increase in the length of this axis.

Penfield himself does not draw any conclusion regarding the structure of the crystal, but, obviously, it might be suggested with as much reason as in any of the previous cases that the olivine radicle is situated on the variable axis. It is further a curious fact that the length of this axis varies directly as the number of magnesium atoms in the compound.

CONCLUSIONS.

In the preceding pages abundant stress has been laid upon the difficulties which stand in the way of current views upon the structure of the crystal molecule; this has been done not with any desire to detract from the interest of these speculations or the great value of the accurate observations upon which alone they can be based, but rather in order to emphasise the difference which exists between them and the geometrical conclusions described in the previous article and at the commencement of the present one.

The latter are rigid, definite and final; the former cannot for the present be regarded as more than plausible speculations; if they represent anything approximating to the truth then the atomic arrangement deduced from the refractive equivalents should be the same as that deduced from the topic axes; this has not yet been tested. Again such speculations should take into account the geometrical laws of homogeneity, and therefore regard the possibility of a given crystal having a structure corresponding to one or other of 229 types now established; but at present the crystals investigated have only been referred to one of the fourteen simplest special cases of these types which were established by Bravais; and to one or other of these types the symmetry of the molecule itself has also been referred. Meanwhile, however, it is encouraging to note that the problem of molecular structure has been attacked, and that at any rate a firm geometrical basis has been provided.

In my own opinion the conclusions to be drawn from the interesting memoirs mentioned in this article are not those which have been suggested by their authors, but rather the point which they implicitly assume. All these researches appear to me to prove nothing regarding the arrangement of atoms rigidly distributed in a crystal molecule, and united by bonds having certain directions in a crystal, but they do prove one very important fact which is equally suggested by the geometrical treatment and by the experi-

mental work; namely, the independence of the parts in the crystal structure as shown by the additive nature of the crystal characters. There is nothing resembling the composition of forces or of motions, each part contributes its own effect independently of the others; morphologically, in the axial ratios; physically, in the refractive equivalents and other properties; this appears to be equally true, whether our attention is directed to the molecule or to the atom. Further, the effects of different atoms or radicles are most marked in certain directions within the crystal, but this may be simply the result of the rigidity of the structure, and does not necessarily indicate the relative positions of the atoms.

All the observed effects are, however, in strict harmony with the nature of a homogeneous rigid structure as defined above. Whatever the parts may be of which a crystal consists they must be uniformly repeated; and whatever motions they possess must be confined in such a manner that each may be regarded as simply juxtaposed beside its neighbours without encroaching upon their domain.

H. A. Miers.

WHAT IS A "TENDENCY"?

IN the interesting discussion which took place recently in the columns of *Nature* on the possibility of the "Inheritance of Acquired Characters," as in all recent writings on the subject of heredity, much use was made of the terms "tendency" and "predisposition". I have searched Darwinian and post-Darwinian literature in the hope of finding an exact definition of these terms, but at present without success. If used vaguely and without any definite meaning, it will be admitted that they have no more value than the "essences" and "humours" of mediæval naturalists; and it seems to me that a clear idea of the meaning of these terms must be the basis of any fruitful discussion of the phenomena of heredity.

What is meant by the phrase that an organised being has a "tendency to variation"? Clearly this is meant to express some property or function of the being in question. Now, with regard to the origin of functions, let me quote from Professor Huxley, not because he favours my views on this question, but because he is the clearest thinker and writer of the day on biological subjects: "All functions, intellectual, moral, and others, are the expression or the result, in the long run, of structures, and of the molecular forces which they exert".[1] I do not think that the soundness of this statement will be generally disputed. The tendency to variation then, in an organism, is a function resulting from molecular forces exerted by the structures of which that organism is composed. The only alternative hypothesis appears to me to be that this tendency is the result of an internal vital force independent of matter; and this hypothesis can hardly be accepted by those who regard Natural Selection as the sole or the main law which has favoured the development of the different forms of organic life.

[1] *Darwiniana*, p. 472.

Now let us take two sentences quoted by Sir Edward Fry from Weismann in the discussion already referred to: "By the term acquired characters I understand those which do not exist originally in the germ as tendencies".[1] And again: "An organism cannot acquire anything unless it already possesses the predisposition to acquire it".[2] It follows, from the first of these quotations, that there are, in Weismann's view, characters which do exist originally in the germ as tendencies; and, seeing these are not acquired characters, they must have been inherited from the original germ of all organic life. This then is obviously the history of all non-acquired characters; and it must be noted that the definition quoted above seems to show that Weismann's idea of a predisposition or tendency agrees with my own, *viz.*, that it is a character, property, or function, of the organism in question. Next, let us endeavour to trace the history of an "acquired" character. Its acquisition requires, according to Weismann, a "predisposition" in the organism acquiring it; if by a predisposition is meant anything definite, that something must be regarded as a property or function; and that function, according to Huxley, is an expression of the molecular structure of the organism. If this view be accepted, it excludes the idea that the function is the direct result merely of external forces acting on the organism; and only two alternative interpretations seem to me possible: either this predisposition or tendency is inherited; or it has sprung up spontaneously, and independently either of the "molecular forces exerted by the structures," or of the direct action of external forces. The latter is of course the hypothesis of a vital plastic force originating independently of matter, a theory which will hardly be accepted by modern biologists of the Darwinian or post-Darwinian school. But, unless we accept this hypothesis, then—if the arguments I have used have any validity—the essential distinction which some have attempted to draw between "acquired" and "non-acquired"

[1] *Das Keimplasma*, p. 514.

[2] *Essays on Heredity*, 2nd Engl. ed., vol. i., p. 171.

characters breaks down altogether. All are the outcome of the hereditary transmission of a tendency or predisposition possessed by the original germ. This hereditable tendency to vary lies at the root of all evolution, and of the acquisition of all new characters and functions.

Whether this tendency to variation in organic germs is equally strong in all directions, or whether, indeed, there is such an unlimited tendency, is a question the discussion of which would lead us beyond the limits of our present inquiry. I have already attempted to point out, in a paper read before the British Association at its meeting in Liverpool in 1870,[1] how difficult it is, on mathematical grounds, to explain the phenomena of Mimetism by the application of the law of Natural Selection acting on an unlimited tendency to variation in all directions. Twenty-five years ago I wrote:[2] "The discovery of this law (Natural Selection) marked an era in the history of natural science, and gave a wonderful impulse to original research. The danger now is that the law will be pressed into services which have no claim upon it; and that, in the hands of injudicious partisans, it will become a hindrance rather than an aid to science, by closing the door against further investigations into other laws which lie behind it. To claim for Natural Selection the main agency in the creation of the countless forms of organic life with which we are surrounded, is straining it beyond its strength. An era of equal importance will be marked by the discovery of the law which regulates the tendency to variation that must necessarily underlie Natural Selection." Are we much nearer to a solution of this problem now than we were a quarter of a century ago?

My chief object in the foregoing remarks has been to show that, accepting Weismann's own definition of "acquired" characters, there is still in these characters a hereditable factor, which renders it impossible to draw any satisfactory distinction between them and "non-acquired"

[1] See *Nature*, vol. iii., p. 30, 10th Nov., 1870.

[2] *Tom. cit.*, p. 33.

characters. It may be that I approach the subject with a prepossession; seeing that, in that branch of natural history with which alone I have any practical acquaintance, the facts within one's own knowledge, as well as those recorded by others, have led me irresistibly to the conclusion that any character or habit once acquired, in whatever way it may have been obtained, is potentially transmitted to the descendants of the individual acquiring it. But knowing that some who have studied very deeply the laws of life in the animal kingdom have come to an opposite conclusion, I should be very glad if these remarks were to bring out from any of them an explanation which would throw light on this aspect of the problems of life.

ALFRED W. BENNETT.

RECENT CONTRIBUTIONS TO THE GEOLOGY OF THE WESTERN ALPS.

A LARGE scale geological map is usually recognised as occupying the same position in the geological study of a country as a trigonometrical survey does in its geography; but it is much less often realised. In mountainous countries it is an ideal at which it is especially necessary to aim, and which is especially difficult of attainment. In regions like the Alps, where much of the ground is covered by snow and ice, by forest and *débris*, where artificial sections or the plough do not come to the aid of the geologist, and where unscaleable cliffs bar his approach to many of the finest sections, a complete map is often impracticable. Moreover, the geological interpretation of the structure of a country may be at any time modified by the raising of the snow line after a dry, hot summer, whereby rocks are exposed to view which have not been seen for decades, and will, on the first winter's snow, be covered for decades again. Professor Bonney has clearly pointed out the reasons why a detailed, petrographical map of the Alps of Switzerland will be long delayed,[1] even if the difficulties be finally overcome at last. And in the Western Alps of Northern Italy lack of accommodation and the suspicions of the frontier guard place further obstacles in the way.

It is therefore not surprising that in the study of the geology of the Alps hypothesis and speculation have been more than usually rampant. Nor is it strange that antiquated theories and unscientific notions still linger amid these mountain fastnesses, with the ghosts and goblins that survive in the minds of the superstitious peasants of the upland chalets. This is especially the case in the Western Alps, which, in addition to the previously mentioned difficulties, have special ones of their own. The Central

[1] *Quart. Journ. Geol. Soc.*, vol. xlvi., pp. 187-188, 1890.

Alps are rendered accessible by roads and railways. The Eastern Alps are the playgrounds of the two foremost Continental schools of geology, Vienna and Munich. The great bow of the Western Alps has no such advantages. The most critical part of the area is a frontier line, which has little hotel accommodation, and attracts few visitors. The military authorities, moreover, do their best to make this number even smaller still. The only geological maps are on a small scale and very diagrammatic. Nor is there any prospect of anything better for much of the area being attempted for some time to come. So the present plethora of hypothesis may long continue. During the past two years numerous important additions to the literature of the area have been made, but these show how far we still are from any agreement upon even the fundamental facts.

In the first place it is necessary briefly to summarise the main outlines of the structure of the Western Alps. As is shown in fig. 1 these consist of a series of concentric chains[1] which sweep round from Lucerne and the St. Gothard through the districts of Geneva, Mont Blanc, Dauphiné, and the Cottians to the Apennines. The most persistent zone is that of the "Zone du Briançonnais," which consists in the main of a band of Carboniferous, Triassic and Lower Jurassic rocks which extends from the St. Gothard to Genoa. To the east and west of this are two zones formed of schistose rocks, and containing many great "massifs" of gneiss. These gneisses were originally described as the oldest rocks in the zones, but they are now known, in many cases at least, to be of later date than the schist and of an eruptive character. The zone to the east is known as that of Monte Rosa; the one to the west as that of Mont Blanc. Outside the latter is a band of limestone composed in the main of Jurassic and Cretaceous rocks. On the north side of the curve there is a zone not represented to the west; this comprises the zone of the Préalpes, including the groups of Chablais, south of Lake

[1] The distribution and composition of these chains have been clearly summarised by Dr. Diener in his *Gebirgsbau der Westalpen*, 1891.

Geneva, and the Stockhorn. To the north of the Préalpes lies the basin of the Miocene deposits of Northern Switzerland, which was supposed to have been occupied by an hypothetical range of mountains—the Vindelecian Chain—in Eocene times.

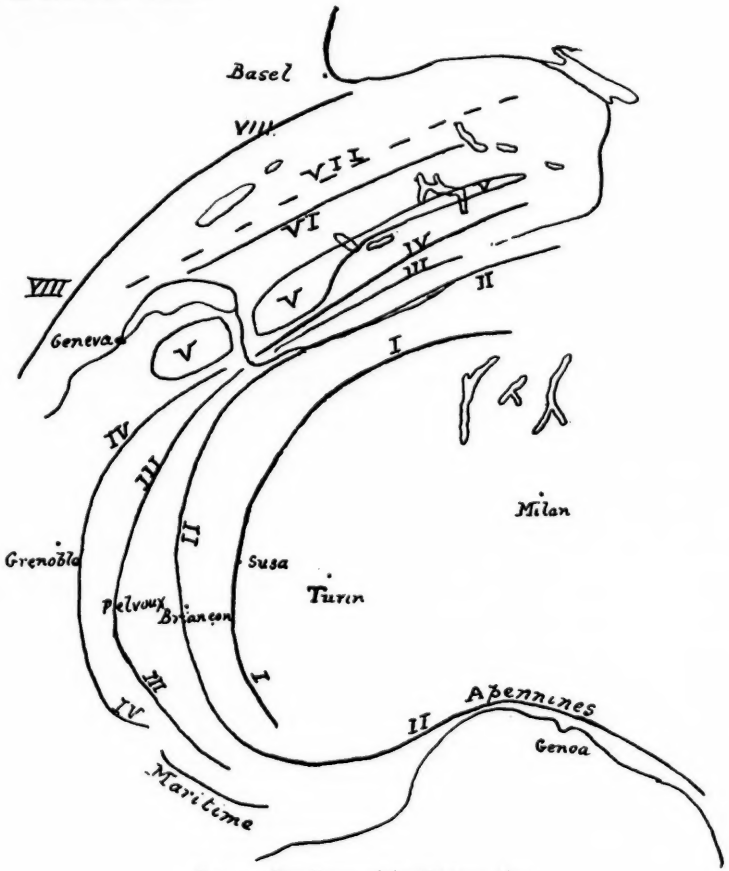


FIG. 1.—The Zones of the Western Alps.

I. Zone of Monte Rosa.
II. Zone of Briançonnais.
III. Zone of Mont Blanc.
IV. The Limestone Alps.
V. The Préalpes.
VI. The Molasse Anticlinal.
VII. The Juras.
VIII. The Vindelecian Chain.

The three problems in the Western Alps that are now occupying most attention, and which must be settled before any permanent advance can be made, are the following: (1) the age, origin and relations of the schists and gneisses

of the zones of Mont Blanc and Monte Rosa; (2) the existence of the hypothetical "Double-Fold of Glarus" which has been assumed to explain the occurrence of older beds over newer ones in the Tödi-Windgallen group; and (3) the origin of the Préalpes and the possible former existence of the "Vindelecian Mountains".

The "Double-Fold" and Dr. A. Rothpletz's alternative explanation (1) have been clearly summarised by Dr. Hume in a recent number of "SCIENCE PROGRESS," so that the question need not be further considered here.

I.—THE STRUCTURE OF THE PRÉALPES.

The Préalpes (of M. Renevier) or the zone of Chablais (of Diener) is one of the smallest of the Alpine zones, but it has given rise to much discussion. The zone forms a narrow band which runs from the western end of the Wallensee to the great bend at the eastern corner of the Lake of Lucerne. It includes the southern slopes of the Rigi and Pilatus, and the whole of the eastern end of Lake Thun. Further west it expands to include a broad tract of country, comprising the Stockhorn and other mountains to the south of Freiburg, and the district of Chablais to the south of the Lake of Geneva. The zone is composed of rocks ranging in age from the Trias to the Oligocene, the characters of which are very different from those of the zone to the south, or the main Limestone Alps. They approximate much more closely to those of the "Zone du Briançonnais," which is three zones to the south. The occurrence of this isolated mass, which seemed so different from and independent of its neighbours, on the north flank of the Limestone Alps, appeared so anomalous that many efforts to explain it have been made. The best known of these is that of M. Schardt in a paper (2) published at the end of December, 1893. He denies the obvious explanation that the remarkable differences between the lithological character of this zone and that of the next to the south may be due to deposition under different conditions. He says there is no evidence for this, and the

change is much too sudden. He points out the resemblances of the rocks of the Préalpes and those of the zone of Briançonnais, and concludes that they were once connected by a great sheet of rocks belonging to this zone, which completely covered the two intervening zones (those of Mont Blanc and the Limestone Alps). It had previously been suggested by Bertrand and Steinmann that the Préalpes were the remains of a great arch like that here suggested. But instead of the arch having risen to the south of the area, as Schardt concludes, they thought that it rose to the north. In that case it would have formed a great mountain range over Northern Switzerland. This has been accepted by many geologists, and named by Gümbel the "Vindelecian Chain" (fig. 1). Schardt, however, shows that there is nothing to the north that could represent the other limb of the arch. He therefore accepts the arch, but makes it cover Southern, instead of Northern Switzerland.

This view has been attacked from two sides. One of the points the great arch has been conjured up to explain is the occurrence of huge blocks of granite in the Flysch of the hills on the north side of the Lake of Thun. Some of these are in size fifty metres cubed. Some of them are so large that they have even been described as intrusive granites. M. Sarasin (3) has studied these boulders in detail, and shown that their existence is fatal to, and not explained by Professor Schardt's theory. The boulders occur in four zones: those of the lowest, or the zone of Ormonts, are familiar to all visitors who ascend the Niesen, on the south side of Lake Thun; the highest occurs typically at Habkern, on the north side of the same lake. Sarasin shows that the materials in all of them have come from the south, and not the north, but that those of different zones have come from different areas. He concludes that at the beginning of the Eocene the region of the Préalpes was occupied by a series of arms of the sea parallel to the Alpine chain, and separated from one another by islands of Jurassic rocks. In these arms of the sea were formed the breccias of Ormonts and the Niesen,

which consist of materials from the north slope of the zone of Mont Blanc. Toward the end of the Flysch a great part of the Alps, including the Préalpes, was elevated and glaciers formed upon the highest peaks. These brought into the basin of Lake Thun masses of the granite of Baveno and Predazzo, of the porphyrites of Lugano, and of the protogines of Mont Blanc and the St. Gothard. This would have been quite impossible if an arch had connected the zones of the Préalpes and that of the Briançonnais, and covered the area from which these rocks have been derived.

Professor Schardt's theory has been attacked from another side in a short but extremely interesting paper by M. E. Haug (4). In this he considers the origin of the Préalpes, and offers another explanation of the resemblances between the rocks of this zone and that of the "Zone du Briançonnais". Bertrand has laid it down as a geological law that folds reproduce themselves along the same lines. Therefore, if we want to find the site of the ancient lines of earth movements in an area, we must look for them below the lines along which great folding may be seen in the rocks at the surface.

Haug accepts this law, and his paper is devoted in the main to applying it to the Préalpes. He shows that the Carboniferous rocks are developed in two basins, an internal one which passes from Liguria to Briançon, through the Tarentaise and the Maurienne, to the Little St. Bernard and along the Rhone Valley. To the west of this is a great anticlinal line, which represents an old pre-Carboniferous mountain range. Beyond this comes an external band of Carboniferous rocks, which were deposited in another synclinal.

In the Permian we get the same arrangement repeated: two bands of Permian rocks deposited in synclinals and separated by a great anticlinal. With the Trias this arrangement is repeated again.

In the next system, however, there is a change, for the whole of the zone of the Briançonnais was then raised as a great anticlinal. The summit was above the sea level,

while shore deposits were formed upon its flanks. Farther away from the ridge deposits were laid down on either side in much deeper water. To the west occurred the "Liàs à faciès dauphinois," and to the east the "schistes lustrés," which, he says, we now know to be "really Mesozoic". Exactly the same condition of things occurred in the Préalpes, where Lias was being deposited on either side of a land area. In the Middle Jurassic part of the Zone du Briançonnais was still above water. In the Upper Jurassic the Préalpes became wholly submerged.

With the Cretaceous we get a repetition of similar conditions over the same areas. Thus where in the Jurassics we had massive limestones, these occur again in the Neocomian. Thus Haug contends that the main characteristic of the geology of this area is the repetition of beds of the same lithological character on the same site at different geological periods, as in the Lias, Dogger and Neocomian.

He says at different times we should have had the same variations along a line across this region at right angles to the zones. Starting from what is now the plain of Northern Switzerland and going southward, we should have found first a shallow-water sea; this would deepen southward until somewhat "oozy" deposits were laid down. Then on the summit and flanks of the Préalpes we should have found shore deposits being formed, and then a repetition of the deep-water beds in what is now the Rhone Valley. These again passed into the littoral deposits on the shore of the Limestone Alps.

Haug therefore concludes that the resemblance between the beds of the Préalpes and those of the Briançonnais is due to the identity of the conditions under which they were formed, and is no proof of an original connection between them, or of the Préalpes being part of an overthrust from the zone of the Briançonnais. He maintains that the fact of the Upper Cretaceous beds of the former being unrepresented in the latter is a sufficient disproof of Schardt's hypothesis. Moreover, the Eocenes occur upon members of the pre-Liassic series in the Préalpes in

a way that shows that the latter were then an old land ridge.

M. Haug therefore accepts the Western Alps as supplying a proof of Bertrand's law. He maintains that the main structural lines were determined before the Tertiary by certain lines of weakness, along which all the later folds have been moulded.

This paper is one of great interest. Its conclusions in no way depend on the truth of Bertrand's law, though this helps in their expression. It certainly appears to offer a simpler explanation of the anomalous character of the Préalpes than that of Schardt's overthrusts and overfolds.

II.—THE GNEISSES AND SCHISTS OF THE WESTERN ALPS.

A more important and difficult question is that of the age of the beds of schists and gneisses which play so large a part in the composition of the more famous Alpine peaks. Upon this subject there have long been contending theories. There is that of most of the Swiss geologists, who contend that many of the schists are altered rocks of various periods, ranging from the Carboniferous to the Jurassic. In opposition to this is the theory that they are all of Archean age; that there is a threefold sequence of ancient gneisses, lower and coarser schists, and upper and finer schists. The former is the view adopted by the Swiss geologists, such as Heim, Schardt, Grubenmann, etc.; the latter is that of most of the Italian geologists, though its most active champion is Professor Bonney.

The subject of the relations of these gneisses and schists has been attacked from different sides during the past year.

The first paper to be noticed is one by Professor Bonney (5), in which he reaffirms the conclusions advanced in his previous paper "On the Crystalline Schists and their Relation to the Mesozoic Rocks in the Lepontine Alps,"[1] and replies to the criticisms of Heim and Stapff upon it.

[1] *Quart. Journ. Geol. Soc.*, vol. xlvi., pp. 187-240, 1890.

The paper deals with two different areas—Altkircke on the north of the St. Gothard Pass, and the Val Canaria and Val Bedretto to the south of it. The southern area is the more easily disposed of. It is described as affording further evidence of the truth of Professor Bonney's view, that the Swiss geologists have confused a set of Mesozoic phyllites with a series of Archean schists. They therefore maintain the Mesozoic age of the latter rocks. It will simplify the question to leave the age of the schists uncertain; for to call them Archean introduces a theoretical view which must be settled on general probabilities, and cannot be definitely decided in the field in this area. But with this reservation it is difficult to compare the arguments used by Professor Bonney in his "Lepontine" paper and Professor Heim's criticisms without feeling that the conclusions against which the former contended cannot be accepted without much fresh evidence in their favour. Professor Bonney now further supports his position, which Professor Heim's criticism has really only strengthened. He shows (1) that the dark-mica schist and black-garnet schist of the south side of the Val Bedretto are the same beds as those of the north side of this valley; (2) that these can be shown on the ground to be continuous with the disthene schists of Nufenen; (3) that as the Triassic rocks contain fragments of the three rocks (dark-mica, calc-mica and disthene schists) with which the garnet schists are interstratified, these cannot be of Jurassic, but must be of pre-Triassic age. Their exact age cannot be definitely decided. But it must be admitted that Professor Bonney has proved that they are older than the oldest fossiliferous rocks in the district.

With the other part of Professor Bonney's paper the evidence is less satisfactory. The Altkircke marble occurs in a series of Jurassic phyllites, and it is claimed by the Swiss geologists to be, therefore, also Jurassic. Professor Bonney, however, contends that it belongs to a much older period. He describes the occurrence of several outcrops of this marble in the Jurassics, and Dr. Stapff states that there are many more. The evidence is admitted by Pro-

fessor Bonney to be very indecisive. He objects that the marble cannot be Jurassic, as its lithological characters prove that it belongs to the series of the "upper crystalline schists". He therefore has to explain the way in which a part of the schistose series has been interstratified with the Jurassics; this he does by assuming that wedges of the older rocks have been thrust up into the newer ones. Cases of this sort of earth movement are now well known in the Belgian coal-fields, so there is nothing impossible in the explanation. But Professor Bonney admits that at Altkircke the wedges "must be unusually long and thin" (p. 291), for they are seen in the tunnel more than 1000 feet below the surface, as well as upon it. He admits also that "the general coincidence in strike between the marble and the Jurassic rocks is singular" (p. 297), and that "the stratigraphical evidence seems at first sight favourable to regarding the marble as merely a peculiar member of the group of Jurassic rocks" (p. 286).

The perusal of this part of the paper does not carry the same conviction as does that of the description of the supposed Jurassic schists to the south of the St. Gothard. When we remember the readiness with which carbonate of lime undergoes crystallisation, it appears quite probable that in the rolling out of some beds of shales containing lenticular masses of limestone the latter should be rendered completely saccharoidal, while the shales have only passed into phyllites. When therefore we hear from Stapff that there are many beds of this marble along the line described by Professor Bonney, and that these are always interstratified with the Jurassics, it seems not impossible that they may ultimately prove to be part of this series.

The question is, however, comparatively a matter of detail. Even if the Altkircke marble prove to be Jurassic, it does not prove the occurrence of Jurassic schists, and does not invalidate the truth of Professor Bonney's main contention.

The second contribution to this series is the most remarkable of all the papers on the Western Alps that have appeared during the year. It consists of two papers

by M. Bertrand, which at once arrest attention by their reactionary conclusions (6). They deal with a region of the Alps not much visited by tourists at the present day, though it was once familiar, as it was traversed by the old coach road across the Pass of Mount Cenis. It may therefore be as well to point out that the area described is situated on the frontier between France and Italy, and extends from the southern end of the Mont Blanc massif on the north to the famous Pass of Mount Génèvre a little to the south of the Mount Cenis tunnel. It therefore includes the northern part of the Cottian Alps, the western part of the Graians, and the eastern section of the Alps of South Savoy. Diagrammatically the area may be described as consisting of three zones concentric to one another from a point somewhere near Turin. The outer or western zone is part of the zone of Mont Blanc; the inner or eastern zone belongs to that of Monte Rosa; while the middle one is the typical area of the zone of the Briançonnais. The last is composed of fossiliferous sedimentary rocks of Carboniferous, Triassic, Jurassic and probably also of Cretaceous age. The two outer zones are composed of unfossiliferous rocks, either igneous masses or altered schists. In the western zone there are the three gneissose massifs of the Pelvoux, the Grandes Rousses, and the Belledonne with their associated schists. The eastern zone consists in the main of a vast expanse of schists with several gneiss massifs, such as that of Monte Paradiso and those recently named the Waldensian gneisses.

When this area was first systematically described by Fournet in his elaborate "Mémoire sur la Géologie de la parte des Alpes entre le Valais et l' Oisans,"[1] issued between 1841 and 1849, the schists of the eastern area were claimed to be representatives of the Jurassic rocks of the central zone, altered by some metamorphic agency. The age was thrown back to the Trias by Professor Lory, whose monograph, the

[1] *Ann. Soc. Roy. Agric. Lyon*, sér. 1, vol. iv., pp. 105-183, 1841; vol. ix., pp. 1-112, 1846; and sér. 2, vol. i., pp. 185-269, 1849.

"Description géologique du Dauphiné" (1861-1864), still ranks as one of the most important works on Alpine geology. For many years, under the influence of Lory's teaching, this view was almost universally accepted. Those who, like Gastaldi, rejected it were actuated mainly by theoretical considerations. In 1887 and 1889 two protests against this view were made after a careful examination of typical sections across the Cottian Alps. These were issued by Signor Zaccagna and Professor Bonney. They showed that the schists underlay the Triassic rocks unconformably, and must therefore be pre-Triassic. They regard them as very much older than the Trias, and assign them to the Archean. Their evidence seemed incontrovertible, and has been supported, at least in affirming the pre-Triassic age of the schists, by Mr. A. M. Davies and the present writer.

Now, however, M. Bertrand, who had previously announced that Lory's view must be abandoned, has gone back to the old idea of the Triassic age of the schists, and especially of that part of them known as the "schistes lustrés". The reasons for this reactionary step are stated in two papers, which are issued together as "Études dans les Alpes Françaises". The first describes the arrangement of the main structural lines in the country and their relation to the great igneous masses, and the metamorphism which the rocks have undergone. The second describes the "Schistes lustrés de la zone Centrale," and discusses their age. It is really the latter subject that is the more important, and the one which may be the more easily settled.

Bonney and Zaccagna have shown that in some places the "schistes lustrés" are unconformably overlain by the basal beds of the Trias, or even Permian. The former author has given a section of the Pass of Mount Génèvre, the accuracy of the facts in which M. Bertrand does not question. It appears as if this one section is quite sufficient to settle the relative age of the two series. Bertrand, however, denies this, and says we must rest our conclusion on the general evidence of all the sections in the district.

He says that elsewhere the structure of the country can best be explained by the hypothesis of the Triassic age of the "schistes lustrés," and that we must therefore adopt some other explanation of this superposition. He himself describes a section at the Col d'Etache in which the "schistes lustrés" appear to be older than the Trias, though they there occur above it owing to an overfold. Bertrand at first thought this was so conclusive that he felt bound to believe that two beds of very different ages had been confused together as the "schistes lustrés". But on re-examination of the ground he finds that the sequence of rocks there may be explained by the assumption of some very complex folds. He does not offer any explanation of the Mount Génèvre section, but dismisses it (p. 153) with the suggestion that some similar arrangement will explain the superposition of the Trias on the schists.

M. Bertrand admits that the Col d'Etache section is "une sérieuse difficulté" (p. 150), and that his explanation is "n'est encore qu'une induction" (p. 152). He certainly introduces a complex hypothesis in order to bring a comparatively simple section into harmony with his theory, and this does not incline one to regard it with favour. He admits that sections of Mount Génèvre and the Col d'Etache give no support to his view of the age of the "schistes lustrés"; he only maintains that they can, with sufficient ingenuity, be reconciled with it. His case rests mainly on his description of the northern part of the country. Mount Jovet and the Grande-Sassière are his favourite illustrations. He represents these as being synclinal mountains in which the highest bed is also the newest; for the rocks which are seen on the lower slopes he thinks pass under the summit, and are continuous from flank to flank. The central summit of Mount Jovet is a typical "schist elustré"; on the slopes there are Triassic rocks. These M. Bertrand maintains are part of a sheet which lies underneath the "schistes lustrés," and are therefore older than these. In the Grande-Sassière he declares that this is shown with diagrammatic clearness. If the facts are as M. Bertrand represents them, then the "schistes

lustrés" of Mount Jovet must be either Triassic or post-Triassic. The mountain, however, has also been described by Zaccagna (7), according to whom the structure of the mountain is totally different. He agrees that the summit is of "schistes lustrés," and the lower slopes of Trias. But he says that the former is a great central mass, and that the Trias was deposited against its flanks. He is as positive that the "schistes lustrés" are older than the Trias as M. Bertrand is of the reverse.

These two mountains therefore supply a crucial test, and it is to be hoped that they will be redescribed by some independent geologist. For while these two authors are in such absolute contradiction as to the fundamental facts of the question, it is impossible for any one to form an opinion without an actual examination of the country.

In addition to his interpretation of the structure of these mountains, M. Bertrand rests his case on the evidence of the general structure of the region. He regards the Alpine chain in this part of its course as formed of a great arch, of which the sides have been so closely driven inwards that the summit of the arch became wider than the base, and the two limbs were inverted. The arch is therefore regarded as a typical Alpine fan. The central line of the Alps Bertrand thinks is occupied by a band of Carboniferous deposits, which lie along the axis of this fan-shaped fold. The beds beside it are inverted, but those at a little distance are inclined outwards, and occur in their normal sequence. This "fan structure" was at one time continually invoked in order to explain away awkward facts in Alpine geology. But recent work has overthrown some of the most classical cases of this phenomenon, and one feels very inclined to look suspiciously on any attempt to resuscitate it. M. Bertrand makes one variation in the theory, which helps him to apply it, but may not help others to accept it. He regards the central gneisses and some of the other massifs of the area as amygdaloid lenticles or nuclei, wedged in between the main folds of the district. Thus, *e.g.*, in the case of Mont Blanc, he represents it as an anticlinal nucleus cropping out on the axis of a synclinal fold. This

view helps in explaining the relations of the main gneiss massifs to the structural lines in the district, which the theory that these rocks are the oldest of the series leaves quite unexplained.

The whole of the first part of M. Bertrand's paper is devoted to the exposition of the "fan theory," for upon it he rests his belief of the Mesozoic age of the "schistes lustrés". For only thus can he maintain that these schists are the eastern representative of the beds which farther west are not schistose. He accepts a statement which M. Termier has called a "general law," that "to the east of the Carboniferous band the metamorphism goes on increasing from west to east". So while to the west of the Carboniferous the Mesozoic rocks are unaltered, to the east he maintains that they soon become altered into the "schistes lustrés". He declares that in many places a complete transition can be traced from the Carboniferous and Trias to the schists; that there is an interstratification of the schists and the Triassic beds; and that we get pockets of the Carboniferous sandstone with traces of anthracite imbedded in the schists. He maintains that the "schistes lustrés" are therefore Triassic. He urges that for some reason the beds on the eastern side of the fold were intensely altered, so that their original characters and fossils have been obliterated, and the beds turned into schists. In some places this metamorphism has gone a stage further and turned the beds that occurred beneath those now altered into schists, into the coarse gneisses such as those of the Paradiso.

M. Bertrand's paper is not an easy one to condense or to criticise. It introduces complexity in places where simple explanations previously sufficed; it flatly contradicts the facts reported by recent workers, such as Zaccagna and Bonney; it is based on theories of mountain structure now discredited; it returns to the views of fifty years ago, and ignores indispensable methods of research. The theory depends on two facts: first, the gradual passage from unaltered sediments to altered schists, especially to the east of the Italian frontier; second, the interstratifica-

tion of true schists and Triassic limestones. M. Bertrand's arguments do not, however, carry conviction on either point. The fact that in several sections the schists occur in immediate contact with the Trias (as shown by Bonney, Diener, Cole, Davies and the writer) and that the two then show no approximation in characters is opposed to the former; while the fact that M. Bertrand makes no effort to employ petrographical methods shakes one's faith in his identification of the last. It is no use criticising M. Bertrand's views without an examination of his principal sections on the ground. For he admits that the best known sections in which the facts are beyond dispute present serious difficulties to his hypothesis. He evades these, however, by stating that sections which are not so well known are so absolutely conclusive as to the truth of his views that we must dismiss the others as exceptional and misleading.

Another long memoir describing exactly the same district has been issued by Signor Zaccagna (7). It is illustrated by a map covering almost the same area and on a scale of a third larger than that of M. Bertrand's, and also by a series of sections which run along much the same lines. The facts shown on the maps and the conclusions expressed in the memoirs are almost as different as they can be. Whereas M. Bertrand denies that there are any rocks in the district older than the Permo-Carboniferous, Signor Zaccagna represents more than half of it as Precambrian. According to the former the central coarse-grained gneiss of the Paradiso is an altered Carboniferous sediment; the latter makes it the oldest member of the Archean. Whereas the former regards the "schistes lustrés" as altered Trias, the latter refers it to the upper member of the Archean. Signor Zaccagna therefore reaffirms for the Central Grains the conclusions announced for the Western Alps in general by himself and Professor Bonney.

Zaccagna's work appears to be a careful and detailed piece of field-work. The map is of course only a small scale sketch, and the sections appear in places to be only diagrammatic. We should be inclined to dispute the

accuracy of details, but that the main sequence in the area occurs in the order in which Zaccagna shows it we do not feel inclined to doubt. On the main question as to the age of the "schistes lustrés," or "calcescisti" as he calls them, his opinion is very decided. He divides the Archean into three zones: the lowest or central gneisses, the middle or mica-schists, and the upper or calc-schists. The two latter were referred by Lory and Favre to the Trias, but Zaccagna is positive that they cannot be referred to any part even of the Palæozoic. He says: "It is not therefore possible to admit the idea of referring the calc-schists to the Palæozoic, more or less ancient, as there is a tendency to do among some modern authors, Carez and Vasseur, Kilian and Termier". He maintains that the calc-schists are so intimately associated with the mica-schists, and the series of "pietre verdi" or "greenstones" which occur in them, that they cannot be separated. He says, therefore, that if the calc-schists are assigned to the Palæozoic, so also must be "all the other rocks which extend, alternating with the calc-schists, even to the contact with the central gneiss".

That two authors, studying the same country, traversing the same sections, and often going together over critical areas, could have arrived at such fundamentally different conclusions is rather surprising. Fortunately, they both of them rest their arguments on the same sections, so that the ground is prepared for an independent survey. Thus Mount Jovet, in the valley of the Isère, has been selected by both authors as a test case. According to Zaccagna, it is an island of Upper Archean schists rising up through the Triassic rocks, which were deposited against its flanks; while, as we have previously seen, Bertrand regards these schists as Liassic limestones resting on a fan-shaped fold of Triassic and Carboniferous rocks, though he admits (6, p. 99) that "ce froissement extrême des assises horizontales du sommet de l'éventail est assez difficile à expliquer". It is difficult to compare Zaccagna's sketch of the structure of Mount Jovet (pp. 229-232) with that of Bertrand without becoming prejudiced in favour of the former.

In Zaccagna's paper we also have to regret the absence of more detailed petrographical descriptions of the rocks, and especially of those assigned to the Permian. Into the general question of the sedimentary sequence described by this author we need not follow him, except in two points. He remarks that he assigns to the Trias "all the compact and brecciated limestones, . . . which Lory in his map considered as the Alpine type of his *Lias compacte*, or Lower Lias, and grouped under the name of the 'Calcaire du Briançonnais'". Further, he believes that a break occurred in the deposition of beds between the Trias and the Jurassic. The latter of these points appears to be established by the author, but in the former he probably goes to an extreme.

A third paper (8) issued last year also deals with the main problem considered by Zaccagna and Bertrand, *viz.*, the age and relations of the three members of the schist series. It announces conclusions totally different from those of these two authors. The paper describes the country to the south and south-east of that considered by them, with the exception of a branch excursion to the Paradiso. This author has concentrated his attention on the "central or basal gneisses".

These had been represented as a continuous line running from north to south, parallel to the main chain, and as being the oldest rocks in the Alps. The author, however, now maintains that they are nothing of the sort; that they are a series of isolated independent masses, and occur at no one fixed horizon in the series; that instead of being altered sediments lying at the base of the series, they are igneous rocks intruded into the schists. The evidence adduced in support of this rather startling change of view is of six kinds. If the gneisses had been the oldest rocks, and the others had been deposited upon them, we should expect to find fragments of the gneiss in the beds above them. Gritty and pebbly beds occur in the overlying schists, but there is among them no trace of materials derived from the gneiss. On the contrary, there occur in the gneiss near its margin included fragments of the over-

lying schists, which have been caught up and altered at the time of the intrusion of the gneiss. In many places the gneiss at the margin is saturated with chloritic and amphibolic material caught up from the schists by the intrusive rock. In addition to this "endomorphic" alteration of the gneiss, there is a well-marked "exomorphic" alteration produced in the beds into which it has been intruded; among others the following minerals have been developed by this contact metamorphism: quartz, white mica, biotite, orthoclase, microcline, oligoclase, garnets, kyanite, epidote, zoisite and sphene. The extent to which the beds have been altered varies enormously; sometimes, when the intrusive rock was small in bulk, it has only affected a few inches on either side; at other times the rocks have been intensely altered for fifty feet from the contact. A second line of evidence is offered by the occurrence of apophyses or dykes which run from the main mass of gneiss into the schists. The best case described occurs in the Angrogna Valley, a few miles from the town of Torre Pellice; it is illustrated by a photograph which shows the white veins of the aplite, ramifying through darker-coloured beds belonging to the schist series. Similar cases of the same sort occur in the Pellice Valley, in the Valley of the Po near Crissolo, etc. A third proof of the intrusive character of the schists is afforded by the mapping of the contact lines, for there is a transgressive junction between the gneisses and the schists.

Another very striking point is the fact that none of the old igneous rocks that cut through the schists enter the gneisses. Both the calc-schists and mica-schists contain many beds of greenstone, which no doubt represent altered igneous rocks, some of which were certainly intrusive. These occur in such abundance in the schists that these are called the "pietre verdi" series.[1] These greenstones are amphibolites and serpentines, and they run through the

[1] It should be remembered that the name "pietre verdi" is used not only for the greenstones themselves, but also for the series of schists in which these rocks are most abundant.

schists right up to the gneiss. They, however, never enter it. Their complete absence from the "basal gneisses" is a fact that has long been known to Italian geologists, and has been used as a test point in the separation of the newer and older gneisses; Gastaldi, indeed, used it as a proof that these "greenstones" could not be igneous, but must be bedded sediments of a type peculiar to the time of the "pietre verdi" series. He says that if they were intrusive they must somewhere have cut the basal gneiss. And it seems impossible to give any other reply to this argument except that the basal gneiss is newer, and not older, than the schists. A final proof is afforded by the fact that the gneiss is remarkably fresh in character, and has not been affected by the earth-movements which have crushed and crumpled the schists around them. It is frequently observed that beds in the centre of a great mass of rock escape alteration, while the margins are crushed and changed. But in the case of these "Waldensian gneisses" thin veins of aplite are quite fresh and uncrushed, while the rocks they traverse have been rolled out into schists.

Having thus attempted to show that the gneisses were formed later than the schists, instead of before them, the author endeavours to decide their age. This is a difficult task, and rests on the classification of the earth-movements in the district, and on the age assigned to the "schistes lustrés". To take the latter first, the author is unable to express any very positive conclusion upon this subject. He remarks that they "are unquestionably pre-Triassic," and that they are probably Palæozoic. He rests the former decision on the sections at Mount Génèvre, where the schists underlie the basement beds of the Trias. Moreover, there appears to be sufficient evidence elsewhere to show that the schists are pre-Carboniferous. The author knows of no evidence which enables the date to be fixed more definitely than this. They may be of any age before the Carboniferous. His objection to throwing them back into the Archean rests on the recent discovery in these beds of some cleaved radiolarian muds. The fauna in this is very imperfectly

known, but it is more specialised than any as yet known from the Ordovician or Silurian. It may therefore show that the schists are Devonian; but no stress can be laid upon the evidence as to age while the Palæozoic radiolaria are as imperfectly known as they are at present.

Though, therefore, the gneisses are later than the schists, this does not help us to assign any definite age to them. For a nearer approximation we have to rely on indirect evidence, and notably on the relation of the gneisses to the earth-movements of the district, and the first appearance of fragments in the sedimentary series. The latter only shows that the Waldensian gneisses are pre-Pleistocene, while, if the evidence of Dr. Gianotti can be fully relied on, the Paradiso gneiss was pre-Miocene. The earth-movements, however, give more definite conclusions. These can be classified into six series; one of these is known to be Miocene, and the Waldensian gneisses were intruded later than this. To the east of the Cottian area the Pliocene beds have been upraised to the height of over 1500 feet, so that great earth-movements in this area happened in the Pliocene. There is, however, no evidence to show to what extent these affected the line along which the gneisses occur. The intrusion of the gneisses may therefore have happened either in the Pliocene, or as the last of the series of earth-movements which occurred between the Middle and Upper Miocene, when the forces that raised the Western Alps attained their maximum intensity. The author therefore concludes that instead of the so-called "central gneiss" of the Cottians being the oldest rock in the district, and according to Gastaldi possibly the oldest in Europe, it is, with the exception of the Pliocene and the drifts, the newest rock in the Cottians.

The largest work issued during the year on the district is a monograph by M. Termier on the massif of the "Grandes-Rousses" (9). This group is situated on the north side of the well-known pass of the Col du Lauteret, opposite the principal mountain mass in France, and to the east of the town of Grenoble. The massif consists of a

series of ridges which run approximately north and south. The ridges are due to folds. The geological structure of the massif is much like that of the others in this district. It has been described as consisting of a central coarse gneiss; then a zone of amphibolic gneisses; and then an upper series of mica-schists. These are referred to the Archean. Above this is a great gap, and the series of deposits recommences with the Carboniferous. Triassic dolomites and Jurassic limestones complete the series.

There is therefore a close resemblance between the general stratigraphy and that of the ordinary Cottian sequence, and when we come to details the resemblances are increased, and there are facts described which support the views of the last paper. Thus the supposed central gneiss is described in one case—the "Massif des Sept Laux"—as an eruptive granulite. Moreover, the coarser mica-schists are described as injected with granulite veins. So that we have here a quite independent confirmation of the view that some of the "central gneisses" of the Western Alps are intrusive in their character.

The date of the intrusions is left undecided. M. Termier thinks they are probably of different ages (p. 33). The dolomites seem to resemble those of Monte Chaberton, but they are unfossiliferous; their age is therefore only determined by their position between the Carboniferous and the Lias.

The Carboniferous rocks are of interest from two points. In the first place, they contain a great number of beds of trachyte (or orthophyre, as it is of course called by the author). The site of the volcanic centres through which these were erupted is discussed in the monograph. In the second place, the metamorphism of the Carboniferous beds is fully considered. On p. 8 we are told that the Carboniferous beds are in places very difficult to distinguish from the Archean schists. M. Termier has, however, subjected them to careful examination under the microscope, and says that this method enables them to be distinguished at once (p. 43).

The last chapter in the work discusses the folds and

earth-movements of the district, which M. Termier says gives a certain support to Bertrand's theory of the recurrence of dislocations on the same lines. Its support, however, is very qualified, for the area supplies several exceptions to the law. It appears, however, that ridges occurred having the same trend as the existing ones in times earlier than any of the fossiliferous rocks of the district. The view that the Alps were modern in their development was once accepted. The Grandes-Rousses, however, supplies only one more to the cases which show that the Alps date back to pre-Carboniferous times.

The occurrence of intrusive gneisses in the Alps is not quite new, for M. Michel Lèvy has shown that the central mass of Mont Blanc is not a Laurentian, metamorphic gneiss, but an intrusive granite which acquired a foliation during solidification. This intrusion, however, certainly happened in pre-Carboniferous times. This view has been confirmed by MM. Duparc and Mrazec not only for the main mass of Mont Blanc (10), but also for its northern outlier, the massif of Trient (11). The centre of the latter also consists of a protogine gneiss; on the flanks of this occur mica-schists and amphibolites which are injected by veins of granulite. The amphibolites offer resistance to the process of injection, so that the granulite is far more abundant in the mica-schists. A plate containing two photographs illustrates very clearly the different types of scenery formed by the two sets of rocks, contrasting the massive irregular crags of the eclogites with the smooth slopes of the mica-schists.

Another paper (12), published in the *Quarterly Journal of the Geological Society*, deals with a single peak in the Cottians. It is an effort to establish a stratigraphical sequence in the main chain of the Western Cottians. Monte Chaberton is a fine dolomite mass on the northern side of the historic pass of Mount Génèvre. It has been familiar in geological literature since 1861, when, in an excursion of the Société Géologique de France, some fossils were found in some limestone boulders on the south-eastern slopes. A further collection of these was made by

Michelotti, and was described by him in 1877. The fossils were determined as of Silurian age, and the schists below the limestones were therefore assigned to the Archean. The late Melchior Neumayr re-examined Michelotti's specimens, which are now in the Pisa Museum, and determined them as Cretaceous. The fossils, however, had never been found *in situ*, and it was therefore very doubtful how far they could be relied on in determining the age of the Calcaire du Briançonnais, of which this Chaberton limestone is the main constituent. There were six distinct theories, which may be tabulated as follows :—

Age.	Michelotti.	Gastaldi.	Lory.	Zaccagna and Mattirolo.	Kilian.	Neumayr.	Davies and Gregory.
Cretaceous						--	—
Upper Jurassic					—		
Middle ,,					—		
Lower ,, (Lias)		—	—		—		
Trias		—		—	—		—
Permian				—			
Carboniferous		—					
Silurian	—						

The authors of the present paper therefore made a further search and found not only better fossils, but the exact horizon from which they come. The fossils confirm Neumayr's view of the Cretaceous age of the fauna. The authors, however, find that the fossiliferous limestone occurs let down by faults into the dolomites, and is therefore of more recent age than these. There can be no doubt that the dolomite is Triassic, and the authors therefore separate from it the coral reefs and shelly limestones under the name of the Clos des Morts Limestones.

The determination of the age of these limestones, and the occurrence of a third fossiliferous zone (the radiolarian phthanite of Cesana) at a lower horizon on the opposite side of the Dora Valley, enables a more definite opinion to be expressed as to the age of the igneous rocks of the "pietre verdi" series. These form one of the most striking features in the geology of the Cottians. They are doubt-

less altered basic igneous rocks, but their age seemed very uncertain, for the evidence was contradictory. The authors now show that there is a treble series of basic igneous rocks in this district. The first occurred between the periods of the "schistes lustrés" and the Trias, for the rocks are intruded through the former, and fragments of them occur in the Trias. A second series is post-Triassic and pre-Cretaceous, for it cuts through the former, and not the latter. The third, including the Variolitic rocks of Mount Génèvre, and the associated rocks, is probably Lower Cretaceous.

In connection with this subject we must welcome a paper in which some very important sections near Genoa are again described. For though these are not in the Western Alps they have exercised a considerable influence on the theories regarding the age of the igneous rocks of the "pietre verdi" in them. Mazzuoli and Issel described these in 1884 in a paper entitled "Sulla zona di coincidenza delle formazioni ofiolitiche eocenica e triasica della Liguria occidentale".[1] Their map showed two series of the "pietre verdi," one in Triassic, and the other in Eocene rocks. The igneous series were parallel to one another, whereas the two sedimentary series were unconformable. It was therefore suggested by two authors that the two sets of igneous rocks were really part of the same series. In this they assumed the eruptive origin of the "pietre verdi," just as the Italian authors had assumed that these were altered bedded sediments. Signor Mazzuoli (13) has now returned to the subject, and stated his views in greater detail. He again urges that the serpentines in the Eocene rocks of Northern Italy are mainly the result of the alteration of a lherzolite at the time of its eruption. The euphotide and diabase, on the other hand, the author claims to be the result of the alteration of a sedimentary rock; he declares they were originally a marl, which had been deposited at a considerable depth below the sea; this was traversed by gases and liquids containing silicic acid. The

[1] *Boll. R. Com. Geol. d'Ital.*, vol. xv., p. 2.

prolonged action of these, especially when favoured by high temperature and pressure, the author thinks metamorphosed the marl into rocks which all English geologists would regard as of eruptive origin. Mazzuoli maintains that they cannot be eruptive, because gradual passages from the "argillaceous schist" into the euphotide (gabbro) and diabase frequently occur. He shows one transition from the "argillaceous schist" into the diabase along the Via di Libiola (p. 7, ii., fig. 3). Neither this diagram, however, nor the paper carry conviction as to the truth of this "anfimorphic" hypothesis of the origin of the gabbro and diabase; while we even feel certain doubts as to whether the "scisti argillosi" with which they are associated are of so late a date as the Eocene.

As with Bertrand's paper we notice the same complete neglect of the microscopic examination of rocks. The real difficulties in the passage from a diabase to a clay slate are therefore not considered. The author does, however, give one chemical analysis, but it does not concern the crucial question. Even if it did, we could learn nothing from it, for it starts with an "insoluble residue" of 84·11 per cent., to which nothing further is done, and ends by lumping together the "ferric oxide, water, etc.," which are determined by difference. In this "analysis" less than 10 per cent. of the rock is determined.

The last paper helps us to realise how different are the points of view from which different schools of workers regard the study of Alpine geology. While in England the igneous origin of gabbro, diabase and serpentine is universally accepted, these rocks are still regarded as aqueous sediments by what appears to be the orthodox school in Italy. Arguments which in England would be taken as final would hardly be accepted in Italy as arguments at all. Moreover, the means of research adopted are very different. Thus we find that in some of the papers noticed the use of the microscope is wholly ignored; and to this neglect of modern methods we have to add in one case an unfortunate provincialism in range, for though the references to literature in it are numerous, these do not

indicate that the author has ever read any work not written in Italian.

When, therefore, both the premises and the methods of argument are so different, it is not surprising that the conclusions are as diverse as they can be, and that the use of the tools of 1830 lead us back to the theories of that period.

Under these circumstances immediate agreement is hopeless. We can only trust that by the patient accumulation of facts; by the abandonment of the construction of maps of complex Alpine regions from traverses along the coach roads, and the effort to unravel difficult igneous series without previous training in simpler volcanic areas elsewhere; and by the constant use of the microscope to detect the differences between rocks which superficially appear alike, the basis of an agreement may be at last attained.

BIBLIOGRAPHY.

(1) ROTHPLETZ, A. *Geotektonische Probleme*, 175 pp., 107 figs., and 10 plates of sections. Stuttgart: 1894.

(2) SCHARDT, HANS. Sur l' origine des Préalpes Romandes (Zone du Chablais et du Stockhorn). *Arch. Sci. Phsiq. Nat. Genève*, sér. 3, t. xxx., No. 12, pp. 570-583, Dec., 1893. The views of this paper are repeated by the author in *Livret Guide Géologique dans le Jura et les Alpes de la Suisse*, pp. 171-195, pl. x. Lausanne: 1894.

(3) SARASIN, CH. De l' origine des roches exotiques du flysch. *Arch. Sci. Phsiq. Nat. Genève*, sér. 3, t. xxxi., pp. 570-603, June, 1894; t. xxxii., pp. 67-101, July, 1894.

(4) HAUG, E. L' Origine des Préalpes Romandes et les Zones de Sédimentation des Alpes de Suisse et de Savoie. *Ibid.*, sér. 3, t. xxxii., No. 8, pp. 154-173, 1894.

(5) BONNEY, T. G. Mesozoic Rocks and Crystalline Schists in the Lepontine Alps. *Quart. Journ. Geol. Soc.*, vol. l., pp. 285-302, 1894.

(6) BERTRAND, MARCEL. Études dans les Alpes Françaises. Structure en éventail. Massifs amygdaloïdes et Metamorphisme. *Bull. Soc. Geol. France*, sér. 3, t. xxii., pp. 69-118, pls. iv., v., vi., 1894. Schistes lustrés de la zone centrale. *Ibid.*, pp. 119-162, pl. vii.

(7) ZACCAGNA, D. Riassunto di osservazioni geologsche fatte sul versante occidentale delle Alpi Graie. *Boll. R. Com. Geol. Ital.*, vol. xxiii., pp. 175-244, 311-404, pls. v., vi., 1892.

(8) GREGORY, J. W. The Waldensian Gneisses and their place in the Cottian Sequence. *Quart. Journ. Geol. Soc.*, vol. l., pp. 232-278, pl. xv., 1894.

(9) TERMIER, P. Le Massif des Grandes-Rousses (Dauphiné et Savoie). *Bull. Serv. Carte Géol. France*, No. 40, t. vi., 119 pp., with map and 6 plates, 1894.

(10) DUPARC, L., and MRAZEC, L. La Structure du Mont Blanc. *Arch. Sci. Phsiq. Nat. Genève*, sér. 3, t. xxix., pp. 74-86, pl. i., 1893.

(11) DUPARC, L., and MRAZEC, L. La massif de Trient, étude pétrographique. *Ibid.*, sér. 3, t. xxxii., pp. 357-372, pl. v., Oct., 1894.

(12) DAVIES, A. M. The Geology of Monte Chaberton. *Quart. Journ. Geol. Soc.*, vol. l., pp. 303-310, 1894.

(13) MAZZUOLI, L. Nuove osservazioni sulle formazioni ofiolitiche della Riviera di Levante in Liguria. *Boll. R. Com. Geol. Ital.*, vol. xxiii., pp. 12-55, pls. i., ii., 1892.

J. W. GREGORY.